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THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

SECTION D—MECHANICAL SCIENCE AND ENGINEERING.

THE work of Section D of the American Association for the Advancement of Science is in mechanical science and engineering. The section devotes itself to showing the advances which recently have been made in the principles and applications of science in regulating and using the forces of nature. The papers which are presented usually deal with what are commonly known as the applied sciences, more particularly with those which are based upon physics and chemistry. Under electricity are included its generation, adaptation and use on a commercial scale. Under heat are included such practical questions as the generation and use of steam. Under chemistry are included the combustion of coal and other fuels, and the production and use of gas for heating, forging and annealing, and for the generation of power in gas and oil engines; while in the allied science of metallurgy the problems of the mining engineer and metallurgist of iron and steel are included. Under hydraulics we find a long list of problems, the advancement of which has been rapid in recent years in the utilization of the results of the work of the sun as a heat agent, and in controlling this same transmuted heat energy when it manifests itself in swollen streams.

The section is to be congratulated on having had as its chairman Vice-President Calvin M. Woodward, of St. Louis, who is

well known in that city as professor of applied mathematics at Washington University, and as the originator of the St. Louis Manual Training School. Throughout the country he is equally well known as 'the apostle of manual training' and as one of the leading educators of the world; his reputation and position in the community enabled him to be a most efficient vice-president.

Professor J. Burkitt Webb, of Stevens Institute, at Hoboken, N. J., was elected as councilor, and Professor George W. Bissell, of Iowa State College of Agriculture and Mechanic Arts, Ames, Iowa, was elected a member of the general committee. Professor William Kent, of Syracuse University, Syracuse, N. Y., was elected member of the sectional committee for five years. The sectional committee consisted of Professor Clarence A. Waldo, Purdue University, Lafayette, Ind., vice-president 1903; Mr. Elwood Mead, Department of Agriculture, Washington, D. C., secretary, 1903; Professor Calvin M. Woodward, Washington University, St. Louis, Mo., vice-president, 1904; Professor W. T. Magruder, Ohio State University, Columbus, Ohio, secretary, 1904–8; Professor Mansfield Merriman, Lehigh University, South Bethlehem, Pa.; Professor J. Burkitt Webb, Stevens Institute, Hoboken, N. J.; Professor H. S. Jacoby, Cornell University, Ithaca, N. Y.; Professor H. T. Eddy, University of Minnesota, Minneapolis, Minn., and Professor William Kent, Syracuse University, Syracuse, N. Y.

VICE-PRESIDENT'S ADDRESS.

The vice-president's address was delivered by Professor Clarence A. Waldo, professor of mathematics at Purdue University, Lafayette, Ind., on the subject of 'Engineering and Mathematics.' It was a statement of the great influence of the engineering colleges upon the teaching of mathematics, and was a strong plea for the rationalization of mathematics and especially for engineering students, and also for illustrating the reality of mathematical formulæ and expressions by examples drawn from engineering practise. By this means it will be found that the student will perceive the utility of mathematical applications earlier in his course, and will not be studying the subject for reasons of either blind faith or stolid obedience. As the paper has already been published in the columns of SCIENCE, we gladly refer the reader to the address itself.

EXCURSIONS.

The section met for the presentation and discussion of papers on Tuesday, Wednesday and Thursday mornings and on Wednesday evening. Tuesday afternoon was spent in an excursion under the auspices of the St. Louis Engineers' Club, to the Eads Bridge, and by special train to the Union Station to inspect the changes now being made in its terminal facilities. The members of the section availed themselves of the kind invitation of the managers of the Louisiana Purchase Exposition, and visited the exposition grounds on Thursday afternoon, where, after luncheon, they inspected the extensive buildings and grounds, and the machinery which was being installed.

PAPERS.

Professor A. S. Langsdorf, of Washington University, presented a paper giving 'Graphic Methods for Determining the Equations of Experimental Curves' and giving means for fixing upon the type of equation to be used and of evaluating the constants of an equation which represents a curve found experimentally. Parabolic, hyperbolic, logarithmic and periodic curves are treated by his method. The paper will probably be published in the *Journal of the Association of Engineering Societies.*

Professor J. L. Van Ornum, of Washington University, St. Louis, Mo., described the results of his experiments on 'The Fatigue of Cement Products.' In his experiments he made tests of cubes and prisms of neat Portland cement, and of concrete, applying different loads at the rate of about four times per minute until rupture ensued, and plotting a curve of results showing the number of repetitions necessary to cause rupture when the load was a given per cent. of the ultimate static strength. The results of the tests of cubes of neat cement show that repeated loads, less in intensity than the ultimate strength of the material, will cause failure. The number of repetitions necessary to produce this effect increases very rapidly for loads less than 65 per cent. of the ultimate strength, and seems to become infinite at about 50 per cent. value. For example, 180 repetitions of the load of 80 per cent. of the ultimate static strength are sufficient to cause rupture. Four hundred repetitions of the 70 per cent. load, or 1,000 repetitions of the 62 per cent. load, or 1,700 repetitions of the 60 per cent. load, or 4,000 repetitions of the 56 per cent. load, or 5,000 repetitions of the 55½ per cent. load, will do the same. The same general law applies equally to concrete. The above results with cement and concrete are, therefore, similar to those obtained by Woehler on iron and steel. The modulus of elasticity of cement and concrete is greatly reduced in value under the influence of repeated loads of the intensities indicated. Prisms $5'' \times 5'' \times 12''$ high were used in this work. The paper will be printed in *The Transactions of the American Society of Civil Engineers.*

A paper on 'The Design of Steel Concrete Arches,' by Professor E. J. McCaustland, of Cornell University, Ithaca, N. Y., was read in his absence by his colleague, Professor H. S. Jacoby. The author calls attention to the lack of clean-cut, definite knowledge as to the action of steel combined with concrete under stress, and particularly in an arch ring subject to moving loads, and states that arches are built with factors of safety ranging probably all the way from 3 to 150. He is of the opinion that we do not so much need new theories as we do an extension of our practical knowledge of the mechanical properties of concrete. He gives an abstract and discusses a graduating thesis on the subject by Mr. W. S. Edge. After briefly stating the theory which formed the basis of the investigation and describing the details thereof, he summarizes Mr. Edge's conclusions as follows: (1) that the graphic method of solution is as accurate as is justified by our knowledge of safe unit stresses in concrete; (2) that an arch ring designed for thrusts due to uniform live loading will be too thin at the haunches to resist stresses due to eccentric loads; (3) that in large spans it is more accurate to use Cain's method of subdividing the arch ring, since it gives, in general, results for thrusts which are about two per cent. greater than will be given by dividing the arch into equal horizontal sections; (4) it is better not to try to use a modified semi-ellipse for an earth-filled arch when the rise is less than one sixth, or possibly one eighth of the span, as the equilibrium curve flattens it too much at the haunches. It is better to take full advantage of the rise by making the arch linear from crown to springing, thereby reducing the crown thrust. (5) The maximum bending moment is not produced by a live load covering one half of the bridge. For the crown section the bending moment is the greatest when the load is about three fifths on the bridge. The greatest positive moment, however, occurs with the arch practically one half loaded. The greatest negative moment occurs when the arch is three fifths loaded. As the result of designing

about fifteen arches, Mr. Edge suggests a method of procedure which he has found to be the most simple, and hopes that some one else will be interested in extending the investigations. The paper will probably be published in the *Transactions of the American Society of Civil Engineers.*

Professor Henry S. Jacoby then gave an account of 'The New Features and Tendencies in Bridge Engineering' which he had observed in his very extensive bridge inspection tours which he has had the privilege of taking during the sabbatical period which was granted to him by Cornell University. He noted the increase in the use of plate girders up to those of 128 feet 4 inches from center to center of supports; that the present tendency in railroad bridge construction seemed to be to get rid, so far as possible, of the overhead portions of bridges; that riveted trusses were now built in spans up to 230 feet; that the length of panels had now been increased to 37 feet, and mentioned bridges over the Monongahela and Allegheny Rivers of the Pratt type, with curved upper chords having only 11 panels in 417-foot spans. The maximum span of simple trusses is still the same as it was ten years ago, the record being held by the Louisville bridge of 546 feet 6 inches. The Pratt truss seems to be in the ascendency in both riveted and pin construction. The author stated that the majority of the masonry now being constructed by the railroads, with one notable exception, is of concrete, and that concrete arch bridges had been built with spans up to 130 feet. Professor Jacoby has evidently improved his opportunities during the past year, and his work as an instructor must of necessity be correspondingly benefited by the opportunities which he has accepted for studying bridges in the drawing-room, in the shop, in the field and in use.

The next two papers presented were from the Ohio State University, at Columbus, Ohio. The first one was by Professor Wm. T. Magruder, and described 'An Hydraulic Micrometer Caliper' which was presented for inspection to the section. This caliper consists of a bronze graduated circle sixteen inches in diameter which is secured to the end of a hydraulic drum connected to a stand-pipe, and so that it can be rotated around its axis. Cross-screws, both radial and axial, are carried by the revolving ring so that pointers fixed in the ends of the axial screws can be brought into contact with the surface of a jet of water issuing from the orifice, and so that by means of a scale on the screws all the coordinates of the jet can be obtained for a distance of six inches or more from the entrance to the orifice.

Professor James E. Boyd and Professor Horace Judd presented a paper describing and giving the results of their experiments with 'Pitot Tubes,' and on 'The Experimental Determinations of the Forms of Water Jets.' The paper describes "a Pitot tube as a simple contrivance for measuring the velocity of water. It consists of a small tube placed in the stream so that the water strikes fairly against one end. Some distance from the end it is bent and connected to a vertical glass tube. The current striking against the end produces a pressure which is measured by the height to which water rises in the glass tube (or, in case of high pressures, by a gauge of some sort). Pitot claimed that this height was equal to the distance a body must fall to acquire a velocity equal to that of the stream. Later observers have thought that this is incorrect, and that the water rose much higher. These experiments were with jets, and showed that Pitot was practically correct. Incidentally it was found that the contracted vein in a jet of water from an orifice in a thin plate is about .785 of the diameter of the orifice, and that the velocity of such a jet is over 99 per cent.

of the theoretical velocity. The paper describes experiments to determine the constants of Pitot tubes. Tubes with variously formed tips and of inside diameters ranging from .162 inch to .007 inch were placed in the jet from an orifice in a thin plate. Each tube gave a pressure practically the same as that in the drum, from which the water was flowing. No change was observed when the tubes were moved back and forth along the axis of the jet, the increase of static pressure back of the plane of the orifice exactly compensating for the diminution of velocity pressure. To determine the velocity of the jet, measurements were taken of the coefficient of contraction, the coefficient of discharge, and of the relative velocities at different distances from the axis of the jet. The mean velocity from a two-inch orifice was found to differ from that in the center by less than .0002. The coefficients were:

Orifice.	Coefficient of Contraction.	Coefficient of Discharge.	Coefficient of Velocity.
2 inch.	.6162	.6112	.992
1.5 inch	.6115	.6119	1.001

As the velocity was practically the same in all parts of a section, the figures for the coefficient of velocity represent the constants of the Pitot tubes, which give readings equal to the static pressure behind the orifices. Similar measurements were made with a short pipe from which the coefficient of the Pitot was found to be .993." The hydraulic micrometer caliper described in the preceding paper was used in making the above measurements.

Professor J. Burkitt Webb, of Stevens Institute, Hoboken, N. J., presented a paper on 'Molecular Velocities,' in which he offered a simple illustration in support of Maxwell's theory that the only permanent state for the molecules is one in which the velocities are not the same for all molecules, but that all possible velocities must be supposed arranged according to the law of probabilities. "Suppose a number of small elastic spheres of equal mass moving in all directions with equal velocities, and consider two of them moving at right angles to each other, and so that sphere B strikes sphere A at the instant that the center of A crosses the path of B, the velocity of A in the direction of B's motion is zero, and therefore all of B's motion will be transferred to A. This will increase the velocity of A from v to $v\sqrt{2}$ Evidently another rectangular blow from a sphere C would increase this velocity to $v\sqrt{3}$, and so on, so that we have in this a proof that an equal distribution of velocities would not be a permanent one, and that the final permanent distribution must depend upon the possibilities of the various phases of collision that may occur."

Professor G. W. Bissell, of Iowa State College, Ames, Iowa, presented a paper on 'Iowa Coals.' He stated that the Iowa coal fields have an area of 20,000 square miles, and include the southwest one third of the state. The Des Moines valley mines are the most active. In this district coal is found at depths of from 100 to 300 feet in veins from 18 inches thick and upwards. The thinner veins are profitably worked in conjunction with the manufacture of brick and other clay products made from the coal shales. Iowa coals are mostly bituminous and non-coking. The average proximate analysis gives:

Moisture	8.08	
Fixed carbon	45.60	
Volatile	38.14	
Ash	8.18	100.00
Sulphur	3.42	

The calorific power of Iowa coals as determined at Iowa State College with the Parr calorimeter ranges from 9,180 to 13,141, with an average of 11,780 B. T. U. per pound of oven-dried coal. From a

number of proximate analyses of Iowa coals, and from determinations of their calorific value by means of the Parr calorimeter, he deduced the formula that the calorific value of an Iowa coal $= (14{,}600C + 12{,}180V + 4{,}000S) \times .01$ B. T. U. The following table gives the results of boiler tests made with Marquisville (Iowa) coals of different sizes, with coke and anthracite nut, and is of interest in showing the prohibitive price of anthracite, and that the fuel cost of generating steam with slack coal is from 30 to 40 per cent. less than the fuel cost with lump, nut or steam coal in the same mine.

Kind of Fuel.	Cost per ton of 2,000 lbs.	Fuel cost of 1,000 lbs. steam from and at 212°.
Marquisville, Slack....	$1.43	14.9 cents.
Steam....	2.35	21.2 "
Nut.....	2.54	21.5 "
Lump....	2.88	24.0 "
Coke, Eastern Foundry.	8.00	60.4 "
Anthracite Nut.....	8.95	52.8 "

Following in this line came a paper by Professor C. H. Benjamin, of the Case School of Applied Science, Cleveland, Ohio, on 'The Science of Smoke Prevention.' As Professor Benjamin was for several years the engineering expert in enforcing the ordinance against smoke production in Cleveland, his conclusions are the result of both scientific attainment and practical experience in dealing with the smoke question in cities. They are as follows: (1) That objectionable smoke from soft coal can readily be prevented; (2) that such prevention will result in a higher efficiency and smaller fuel bills; (3) that all new plants should be subject to permits issued by proper city officials; (4) that educational and legal measures combined should be used in cases where the evil already exists; (5) that the control of such work should be in the hands of properly trained engineers who understand the whole subject thoroughly; (6) that the people of each community must see to it that they are protected from this evil as from poor drainage and dirty streets.

Professor William T. Magruder, of Ohio State University, Columbus, Ohio, presented a paper entitled 'A Producer Horse Power—A Proposed New Unit.' After reviewing Watt's unit for a boiler horse power, and that adopted by the Philadelphia Centennial Commission, he stated that the rapid introduction of the use of gas-engines using blast-furnace gas or producer gas leads to the suggestion of a unit for the horse power of a gas producer similar to the unit for the horse power of a boiler. As some gas engines are now delivering a brake horse power for the generation of 10,000 B. T. U. per hour, and a few are doing 10 to 15 per cent. better than this, he suggested as a proposed new unit that of a 'producer horse power.' He defined it as 'the generation in an hour of sufficient gas at 60° F. to produce 10,000 B. T. U. when burned to water and gas at 60° F., or its equivalent.'

Before availing itself of the invitation of the St. Louis Engineers' Club, the section listened to a paper by Mr. A. P. Greensfelder, assistant engineer of the Terminal Railroad Association of St. Louis, on 'Recent Improvements at the Union Station at St. Louis.' The paper was illustrated by the plans for the improvements which are now under way, showed the necessity for them for handling the passenger and freight business of St. Louis, and described in some detail the methods which had been adopted for changing the tracks and moving all the express company buildings, while operating over 1,100 passenger cars each day. Incidentally, the paper showed the advances which have been made in terminal railroad facilities by the use of applied science.

AERONAUTICS.

The program for Wednesday morning was made up of a series of papers on problems connected with aeronautics. Professor J. Burkitt Webb, of Stevens Institute, Hoboken, N. J., presented two papers. One was on 'The Flying Machine Problem,' in which he showed among other things that 'for rapid flight a considerable altitude is favorable.' The second one was on 'Practical Artificial Flight.' The author stated that the inventor should aim for the most practical results and should attack the main difficulties at the start. To this end, he suggested that the question of motive power be eliminated, and that power be supplied from a trolley or from an overhead source, and that the endeavor should be to develop a machine which can slowly and surely rise from the ground and as slowly and safely descend again; and which should be controlled by an automatic balancing device.

Mr. A. Lawrence Rotch, director of the Blue Hill Meteorological Observatory, Hyde Park, Mass., read a most interesting paper, which was illustrated by the stereopticon, descriptive of the 'Exploration of the Atmosphere as Practised with Kites at the Blue Hill Observatory since 1894.' The methods employed and the results obtained are in part described in the appendix of the 'Smithsonian Report' of 1900, and the later technical details will be published in the *Annals of Harvard College Observatory*, Part III. of Volume XLIII.

'The Aeronautical Contests at the World's Fair, St. Louis, 1904,' were outlined and discussed in three papers presented by Professor Calvin M. Woodward, Washington University, St. Louis, Mo., Mr. A. Lawrence Rotch, director of Blue Hill Meteorological Observatory, Hyde Park, Mass., and Mr. Willard A. Smith, chief of the transportation exhibits, and in charge of the Department of Aeronautics, of the St. Louis World's Fair. All three gentlemen are members of the committee having the subject in charge. Professor Woodward introduced the subject. Mr. Rotch described and illustrated with the lantern the most successful dirigible balloons and flying machines, some of which are likely to be tried at St. Louis, and discussed the regulations for these experiments as drawn up by the committee. Mr. Smith continued the subject, discussing it in detail, and describing the facilities which would be offered to contestants for inflating their balloons with hydrogen gas, 97 per cent. pure, made by a new English process, which is guaranteed to deliver 25,000 cubic feet of hydrogen for the combustion of one ton of slack coal. The method of generation, it was stated, will consist of the dissociation of steam by incandescent iron shavings, and the revivification of the oxide of iron so formed by producer gas.

The last paper of the morning was by Mr. Octave Chanute, on 'Aerial Navigation,' and was a most able one. After calling attention to two probable solutions of the problem, he described what has been accomplished with balloons and flying machines, the evolution and limitations of such apparatus, their limited uses, and discussed the prospect of any one winning the prize offered by the World's Fair Commission. It is published in the March *Popular Science Monthly*. The series of papers being by noted specialists of high scientific attainments were greatly enjoyed by all those who availed themselves of the privilege of hearing them.

THE HYDROLOGY OF THE MISSISSIPPI RIVER.

The Wednesday evening program included in its scope the entire Mississippi River Valley, beginning with a paper by Professor C. W. Hall, of the University of Minnesota, at Minneapolis, Minn., on 'The Stream Flow of the Upper Mississippi

River' and ending with a paper by Mr. James A. Seddon, of St. Louis, on 'The Lower Mississippi River.'

Professor Hall's paper was illustrated by a number of lantern slides showing the head-waters of the Mississippi, the geological formations of the valley and a study of the currents and the flow of the waters of the river.

A paper by Judge R. S. Taylor, of Ft. Wayne, Ind., a member of the Mississippi River Commission, was then read on 'Levees, Outlets and Reservoirs.' He stated that the alluvial valley of the river below Cairo contains 29,790 square miles of lands subject to overflow in its natural state; that it is all capable of protection and reclamation by levees, which has been going on for nearly 200 years, except a small area at the foot of each drainage basin, which has to be left open for the escape of surface drainage. The existing lines of levees are about 1,350 miles long and about 80 miles remain to be constructed to complete in length the main river system. In few places, however, are the embankments as high and as strong as they should be for the greatest safety. 'The potential high water of floods to come' has been the subject of much study and discussion. The nearest approach to a standard has been that the levee should be three feet above the highest previous flood line in that locality.

The flood of 1897 made 38 crevasses having an aggregate width of about 8 miles; the flood of 1903 made 9 crevasses, having an aggregate width of about 3 miles. The levees in place in 1903, if no crevasses had breached them, would have protected about 26,000 square miles from overflow. Of that area a total of about 3,000 square miles was overflowed in consequence of the crevasses which took place, which is less than one eighth of the entire area which the existing levees could and would have protected if they had all been high enough and had held their places. In the phrase of the target-shooters, they accomplished 87½ per cent. of success out of a possible 100. The levee system is at this moment in the very crisis of its history. It has demonstrated the possibility of its usefulness. It wants just the last grand effort to carry it to completion. We ought not to think of the diversion of any part of our resources to any other work while that remains unfinished. During the flood of 1903 the existing levees protected from overflow seven eighths of all the lands capable of protection. If great floods should come once in five years, and we should never do any better than we did last spring, this would mean that there would be an average annual inundation of 2½ acres out of every 100 acres. This would seem to show that the present system of levees is successful. The author does not believe in the successful protection by outlets and reservoirs, and paid their advocates the compliment of a polite refutation of their arguments. The paper will be published by the Werner Co., Akron, Ohio.

He was followed by Professor Lewis M. Haupt, of Philadelphia, Pa., who referred to the law of 1879 and to reports of the 'Board of Engineers.' He quoted the resolution of congress of 1891 that 'no portion of the appropriation then made should be expended to repair or build levees for the purpose of reclaiming land, but only when it may afford ease and safety to the navigation and commerce of the river and deepen the channel. He stated that it was shown, by a comparison of surveys made at an interval of twelve years, that the bed had risen about four feet, and that the banks above low water had caved in to a large extent. He urged that the law be amended so as to provide for specific appropriations for levees to protect the waste and swamp lands, which, he claimed, were quite as de-

serving of national aid as the arid lands of the plains. He advocated a more thorough system of drainage by the removal of the obstacles and bars in the section below Red River. He discussed the statements made as to the effect of crevasses, showing from surveys that they are of great benefit in reducing the flood stages and improving navigation, as well as in adding extensive tracts to the arable lands of the state and nation. By removing a large percentage of silt from the river, they also retard the gulfward movement of the bars and flatter slopes which contribute to flood heights. By the natural process of hydraulic grading not less than 150 square miles have been deposited above the gulf level within thirty years. To have filled this up by dredges at ten cents per yard would have cost $1,500 per acre, which would have been prohibitory. He also dwelt on the need of removing bars from the front of all passes, by a curved form of jetty, simulating and applying the action of all streams in creating the deep-water pools found in their concave bends. Thus vessels could freely navigate all the passes, while at the same time the floods would be lowered and the sediment be deposited on the opposite or convex bank.

Colonel J. A. Ockerson, of St. Louis, with the aid of stereopticon illustrations described the work of the Mississippi River Commission, of which he is a member. He discussed some of the physical characteristics of the river and the subjects of flood control and channel improvement, and showed scenes along the river from its source to its mouth, including the levees and levee building, crevasses, and the hydraulic dredges used in the channels and the methods of removing obstructions to navigation.

The last paper of the evening was on 'The Lower Mississippi River,' by Mr. James A. Seddon, of St. Louis, Mo. He stated that the word 'river' is a geographical and not a physical term. That, unlike the tidal rivers, the Mississippi is a power that has made its valley and is master of it. The great flood has more than ten times the power of Niagara in its flow to the gulf. He discussed in considerable detail the physical conformation of the valley, showed that the river has an excess of power to carry its sediment, and stated that the only place where the Mississippi River has formed a bar by dropping this sediment is where it meets the waters of the gulf. At the mouth the flow can no longer carry its sediment, as it is too weak. In the valley the flow is too strong and it chokes itself up and spreads out in shallows. He discussed the subject of dykes and bank protection and gave many interesting facts concerning levee history. The author is in favor of a reservoir system of protection, and stated that this would give the bottom lands a certain flood protection, while emptying the reservoirs at the time of low water in the river would triple its depth, and the cost of the work of reservoir construction would have been a little more than half of the $80,000,000 which has been spent on the lower Mississippi. By this means the river would become a deep waterway which would not stop at the Ohio River, but continuing up the Illinois River through the Chicago drainage canal, would join the lake and gulf commerce. He is of the opinion that what is most needed in this case is a statesman to see 'that the river and harbor bill carries a responsibility that will produce results with its expenditure.'

This series of papers gave as complete a résumé of the subject as the time allotted would permit, and showed what a wide diversity of opinion there is among scientific experts on this extremely important problem in civil engineering and hydraulics.

The Thursday morning program was, as

usual, well filled with several papers which had been left over from Tuesday, and with the remaining papers of the program. One of these was by Professor Frank B. Williams, of Union College, Schenectady, N. Y., on 'Methods of Determining the Coefficients of Elasticity.' By loading a beam supported at its ends at two points equidistant from each other and from the ends, and thereby eliminating the cross shear, the coefficient of linear elasticity can be determined by measuring the deflections. Knowing *E*, the coefficient of elasticity for shearing is obtained by the formula given by Professor Merriman.

General E. W. Serrell, of West New Brighton, N. Y., followed with a paper on 'A Proposed Method of Building the Mandingo Ship Tunnel,' through the Cordillerian range of mountains in Central America, where the distance from sea to sea is but twenty-nine and one half miles. The Gulf of San Blas and the magnificent harbor of Mandingo are at the north end, while directly south, behind the Pearl Islands, within the Bay of Panama, is another harbor. The mountain range averages about 1,520 feet high. The proposed ship tunnel is to have portals 300 feet high. The length of the crown of the tunnel will be less than five miles. Instead of the shales found in the lines of the two other proposed routes for ship canals across the isthmus, the geological formation at this point has been investigated by an expert geologist, who states that the rock extends across the isthmus, that it is very uniform, strong and in every way suited for tunneling. Tested at the Watertown Arsenal, it was found to be stronger than Quincy granite. Analyzed at the geological laboratory at Washington, it was found that hornblende predominated in the granite. The canal-tunnel will be a straight line from sea to sea, and therefore capable of passing a ship of any length. The paper discussed the elements of the cross-section of the tunnel, the method for its construction, using three headings and understoped as well as open benches, and nine overhead tracks to remove the debris, and it is stated that the 18,000,000 cubic yards in the tunnel and the 37,000,000 cubic yards of excavation outside the tunnel can all be made for less than $100,000,000. It is estimated that the work can be completed in two years, although three years have been allowed.

Considering its good geological position, the excellence of the harbors available, the abundant supply of water at sea-level, no locks to delay passage of shipping, and more than ten times the capacity for business, as compared with any other proposed isthmian canal, it would be cheap at three or four times the cost, to say nothing of the short time which will be required to build it.

Before adjourning on Thursday to enjoy the hospitality of the officers of the World's Fair, the section had the privilege of listening to Lieutenant G. L. Carden, U. S. Revenue Cutter Service, on 'Some Topics Connected with the Machinery Department of the World's Fair.' The author is superintendent of 'arsenal tools' in the department of machinery at the fair, and was sent abroad and secured many of the foreign exhibits of machinery.

The section chose Professor David S. Jacobus, of Stevens Institute, Hoboken, N. J., as its vice-president for the next meeting; and, on nomination to the general committee, he was duly elected.

To say the least, the program of Section D was very full. A large number of the papers were by the leading experts of the country on the subjects discussed. It is questionable whether the interests of science are the better advanced by permitting a more lengthy and general discussion of the papers presented, than by having the

other sides of the same subject formally presented by some one who may not fully agree with the preceding author, but who has had time to prepare a written paper defending his position and advocating his opinions. While persons may differ in their opinions as to which is the better plan, the consensus of opinion of the members present was that the program presented gave them much information and food for thought. Many branches of mechanical science and engineering were touched upon, and while special emphasis was put upon those sessions devoted to aeronautics and hydrology, it was thought that the place and its surroundings warranted it.

The attendance at the meetings of the section has been excelled in recent years only by the 1902–3 Washington meeting. It is thought that this is encouraging for the future of the section. It is to be hoped that the members of the association connected with the section will show their continued interest in it by their attendance and by presenting papers at future meetings.

WM. T. MAGRUDER,
Secretary.

*A REPLY TO RECENT STRICTURES ON AMERICAN BIOLOGISTS.**

A NOT uncommon, though possibly more or less indefinitely formulated, opinion has recently found an expression in print† to the effect that American systematic zoology has degenerated into a mere recording of minute facts, instead of being a study of problems; in other words, that it has been reduced to a somewhat low-level, though possibly sometimes useful, craft, and has lost caste among the sciences.

It must be admitted at the outset that this criticism is deserved to a limited extent. If we take ornithology as an example, what are the results of our labors in this country? Look over the long files of the *Auk* and see what they contain: An astounding and in many ways admirable record of facts relating to the distribution of our birds, their habits, their specific and subspecific characters! The refinement and acumen of discrimination with regard to the latter have reached a high degree of development, and it is doubtless true that the birds of North America are better known than those of any other part of the globe of even approximately similar extent. Our collections of native species are vastly larger and more complete than those of any other country and our methods and technique, both of collecting and of recording, greatly superior to those of the rest of the world. And the work goes on unceasingly, and the details are being more skilfully and accurately and voluminously elaborated every day. In fact, we are working so fast and so well that we have left the rest of the ornithological world far behind. Some of the younger European ornithologists are trying to catch up, but they will never be able to do so because the North American material can only be had here and because we have gained such a lead in the race.

But for what purpose are we accumulating all this minute detail, this enormous material? What are we straining our best faculties, our acknowledged ingeniousness, for? Thus far we have but little to show that would give a satisfying answer to these questions. On the surface, at least, it looks as if we were following these pursuits chiefly for their own sake, for the satisfaction of mere accumulating, for the exercise of these mental faculties. To the outsider it must certainly appear as if we regard the work we are doing as an end, not as a means towards an end. The ques-

* Read at the Twenty-first Congress of the American Ornithologists' Union, at its meeting in Philadelphia, November 18, 1903.

† Talcott Williams, 'On the Skirmish Line of Science,' *Booklover's Magazine*, II., November, 1903, p. 458.

tion then arises: Have we really, in our eager pursuit of the details, lost sight of the final object of our studies, which alone can justify the expenditure of brain work and money; and have we thus degraded our science to a mere sport or a brilliant juggling with facts and words? Have we forgotten that the problems are the essential part of science and that the records are only the tools with which to work out the problems?

Thus far there are but slight indications on the surface that the higher problems have attracted much attention, and I am afraid that many of us must plead guilty to having groped in the lower regions so long that we almost forgot that there is something higher. Nevertheless, any one who has the opportunity to look below the surface must be aware that a notable amount of thinking and theorizing is going on without causing much outward commotion. While this holds true to a slight extent for the whole range of problems, it is particularly so with regard to a certain limited class, referring, as I do, to the problems more or less intimately connected with the question of life zones or the zonal distribution of life. Here, thanks to the brilliant work of some of the most prominent scientists of the American Ornithologists' Union, considerable progress has been made in the right direction, and more may be confidently expected in the near future.

It is not difficult to demonstrate just why this class of problems should first receive attention. The explanation is that not only have the requisite facts been recorded on an unprecedented scale and with a clear understanding of the requirements of the case, but nearly all the material necessary for at least a partial solution of the problems are available within the boundaries of this country. The question, up to a certain point at least, is a local one, viz., the interrelation between the North American biota and the various zonal areas which its component animals and plants occupy. Up to this point our scientists will be able to solve the problems. It must be conceded, furthermore, that the truly monumental way in which the material is being gathered, recorded and elaborated makes it possible for them to construct upon it a philosophic building which shall be more enduring than the ephemeral structures of past times. We may confidently look forward to the establishing of proof where formerly we had only uncertain theories and hypotheses.

Just here we have reached the point where we become aware of our limitations and their cause. We have the means of approaching and solving the local problems and questions in so far as they can be elucidated by local work, but we are utterly shut out from attacking the larger, more universal problems, without which we shall have to submit to the stigma of being mere *sciolists*, a name applied the other day to American biologists generally by a reviewer of the achievements of American science.

Whether we accept a holarctic region, or recognize the nearctic and the palearctic as two separate regions, no one will now deny that a great portion of the biota of the northern parts of the new and the old world is intimately related. But when it comes to the questions as to the extent and the degree of this intimacy; whether their faunas and floras have a common origin; or whether they are a blending of two or more biotas, and in such a case, where each component part originated and how the blending took place, by what routes and at what time—when it comes to these and similar questions, we find that opinions and theories are digressing in all directions. If we ask ourselves, in what genetic relation do the animals and plants now inhabiting the northern world stand to those living before them in the same territory; whether

the forms which to-day occupy a certain region are descended directly from those whose bones we find in the strata underneath them, or have originated in some far distant continent from ancestors indigenous there, we meet again a distressing amount of uncertainty and diversity of opinion. And if we inquire into the reason for all this controversy, this lack of agreement among biologists, one cause stands preeminently forth as responsible, viz., *insufficient and defective records!*

Thirty or forty years ago the biologists, with an almost childish faith, believed that they had gathered all the material that was to be had, and that they would exhaust the supply of facts in a very few years. Europe, according to them, was thoroughly explored, the records were complete; northern Asia had just been covered by the magnificent expeditions of Middendorff, Schrenck, Radde and others; the biotic secrets of North America were divulged in the Pacific Railroad reports, the Mexican Boundary reports, the reports of the expedition west of the 100th meridian, and of the survey of the territories. They conceded that a few more species might be expected to turn up in the interior of Africa, but these, it was thought, would not be able to alter conclusions materially. And so they proceeded to speculate and generalize, to pull biology out of the mire of mere recording and gathering of facts into the regions of real science. But unfortunately, although theories and hypotheses multiplied, they nearly all led in different directions, and each philosopher came to results at variance with those of the others, according to the kind of material or the portion of the record he happened to get hold of.

Far be it from me to speak lightly either of the records and material gathered by the men of that generation, or of their generalizations. It was not their fault if the ultimate results have been disappointing. Many of their records are of permanent value; a great deal of their material still serves as foundation for our present work; some of their conclusions and theories have proven to be correct. Without them we were not standing where we are now. The fault lies with their successors who considered the preliminary work finished and who failed to continue it systematically and symmetrically.

Shortly after the period alluded to it became painfully clear to biologists that the amount of facts, material and records which had been gathered was not only a mere handful as to numbers, but even more hopelessly defective as to accuracy and minuteness of the data. With the opening up of the world by means of improved facilities of communication, the enormous mass of new material representing unexpected forms in endless number fairly swamped the systematic biologist during his work of recording and describing. This flood of new species and genera naturally affected the scientists of Europe most, as it accumulated in the museums of countries which not only previously had colonial possessions all over the world, but now by the division of Africa despoiled a whole big continent of its most striking novelties. The American biologists, on the other hand, whose field was in nearly all cases limited by the political boundaries of the United States, were shut out from the rest of the world and reduced to a more intensive cultivation of their own area. The result has been curious in more than one way. On the one hand, our development became defective, because we lost touch with the outer world and so in a measure were left behind; while, on the other hand, we extended the accurate, *i. e.*, scientific, knowledge of our own field far beyond that of the rest of the world.

This, then, is the standpoint we occupy

to-day: We are still confronted by the same problems which our predecessors failed to solve because of their lack of definite and detailed knowledge of the facts. With a view to their solution we have gathered a material which, for technical perfection and minute accuracy, is unrivaled. It has but one fault—it is terribly lopsided. We have or are in a fair way of obtaining shortly most of the data relating to the nearctic region, but we have not a scrap of the right kind of material relating to the other half of the northern world. Nor is the right kind of material in existence anywhere at present; for while there is preserved in the numerous museums of Europe a large number of specimens, and while the literature contains a vast accumulation of records, neither the data accompanying the former nor the observations contained in the latter are, as a rule, so precise or so detailed as now required. This widely scattered material, in addition to its insufficiency due to superficial and haphazard collecting, is distressingly uneven in quality. Moreover, it has not been worked up according to uniform methods, nor by workers occupying the same viewpoint. Its component parts are not only uncorrelated, but they are at present utterly impossible of correlation.

Thanks to the example set here, Europe is just beginning to realize the fact that she has neglected her own fauna. Some of her more wide-awake biologists have recently attempted to grapple with the problems I have alluded to above, but they have not advanced much farther than to formulate them. They have found their *records far too insufficient and defective.*

Lest I be accused of exaggerating let me quote what I wrote more than two years ago in a review of the attempt, by a prominent European biologist, to generalize from the incomplete data at hand. After having said that one of the distinct merits of his work was that it revealed the defects in our knowledge, I continued:

"It is a kind of stock-taking by which we find out just how our business stands. It must then be admitted with regret that the status is not as satisfactory as one might have reason to expect. There is yet a great uncertainty as to the exact and detailed distribution of many of the larger and more important animals in the Arctic regions and in Europe. The grosser facts are known of course in a general way, but they are not sufficient for the purpose. The finer details are still unknown, or if known in some isolated cases are unavailing because they are as yet only isolated."*

This statement has remained unchallenged ever since and but little has been done to remedy the defects in a comprehensive way. What is true of Europe is no less true of Asia. Let me recall to you that a distinguished member of this union, in a paper published during the present year and dealing with a single class of vertebrates only, found himself obliged to bewail his impotency to settle important questions by such statements as these:

"Material from northern Europe available for comparison with the Siberian series is too scanty * * * to be of any importance. * * * "

"Also material is lacking in sufficient quantity to give much new information in respect to the supposed difference. * * * "

"But lack of material prevents a critical consideration of the subject. * * * "

"Without other material it is impossible to compare the present series. * * * "

"In the absence of specimens * * * it is provisionally referred. * * * "

And so forth no less than eight times in the same paper under eight different

* Scharff's 'History of the European Fauna,' *Amer. Naturalist*, XXXV., 1901, p. 113.

species.* I may add that he had the contents of all the leading museums of our country at his disposal.

Another member of the union, who also brought together all the available material from the American museums, published a monograph of a holarctic genus of birds last year.† He recognized 35 different forms by name, 22 from the nearctic region and 13 from the palearctic region. As a basis for this work he had no less than 2,150 specimens, a material which, if it had represented evenly the range of the genus, might have been sufficient to give an exhaustive account of the various forms and might have led to valuable generalizations with regard to their origin and their distributional migrations, but the ridiculous inadequacy of the palearctic material for the purpose may be plainly seen when I state that while the American specimens at his command numbered 2,108 specimens, or an average of over 95 specimens per recognized form, the European and Asiatic material consisted of 42 specimens, or 3¼ specimens per recognized form. Of three of the latter there is not a single specimen in any American museum.

One more striking example, this time derived from the class of mammals. The only museum in North America which has made strenuous efforts to obtain palearctic mammals, and which by all odds contains the largest material from the holarctic region, possesses about 94,000 specimens from the nearctic, as against about 3,300 from the entire palearctic. It is safe to say that this enormous discrepancy is even excelled in the other museums. A similar census of the birds in the same museum was not practicable, but it is perfectly safe to say that the proportions are nearly the same, or thirty nearctic specimens to each palearctic. The discrepancy is the more marked when we consider that the area comprised in the palearctic region is nearly twice as large as that of the nearctic, so that area for area the palearctic material in our museums is scarcely one and two thirds per cent. of the total holarctic material.

It is unfair, therefore, to blame the modern American biologist for his failure to enter a higher philosophic sphere. He has the ambition to do so, he has also the ability; moreover, he has done part of the preliminary work and done it exceedingly well. But as yet he is without the means.

And now, how can this unfortunate condition be remedied?

There is only one way, viz., *the acquisition of more and better palearctic material and records,* collected by professionally trained observers; worked up together with and conformably with the nearctic material and records already gathered and elaborated with such signal success. Because of the possession of the latter it follows of necessity that the American biologist should also gather and elaborate the former. The work already done preeminently qualifies him to carry the whole to a satisfactory conclusion. He has done the first part well; he will do the remaining work equally well, if only given the chance.

It may seem strange to call for more material and more records in reply to the accusation that we are losing ourselves in that very kind of work. It must be borne in mind, however, that what is here called for is not the insatiate, indiscriminate accumulation with no clear purpose in view, but a well-digested, premeditated search for the material which bears directly on the problems already outlined and which experience has shown to be indispensable for their solution.

The fact is that we are not losing our-

* J. A. Allen, *Bull. Amer. Mus. Nat. Hist.*, XIX., 1903, pp. 126, 129, etc.

† H. C. Oberholser, *Proc. U. S. Nat. Mus.*, XXIV., No 1271, 1902, pp. 801–884.

selves as alleged; it only appears so to the outsider. Yet, it is necessary that our higher duties should be held up to view 'lest we forget.' Moreover, the time has come for the gathering of the new material unless we are to sink back into a shallow rumination of the old. The American biologist stands ready to expand his dominion into the old world, if he be given the means, and when he shall be through with his work, the facts and records will be in such a shape that the philosopher can rear a structure upon them that will stand.

The means by which he may be put in this enviable position have been set forth in another connection* and need not occupy us here.

Leonhard Stejneger.
Washington, D. C.,
November 14, 1903.

SCIENTIFIC BOOKS.

The Positive Philosophy of Auguste Comte. By L. Lévy-Bruhl. Authorized translation, to which is prefixed an introduction by Frederic Harrison. New York, G. P. Putnam's Sons. 1903. Pp. xiv + 363. 8°.

Anything that will help to make the philosophy of Auguste Comte known to the readers of English can not fail to be useful. The English translation, therefore, of a work on that subject by such a man as M. Lévy-Bruhl, the well-known author of the 'History of Modern Philosophy in France,' and who 'writes as a student and not an adherent of Comte,' is especially welcome.

It will probably be one day regarded as the most remarkable anomaly in the history of science that the work which formed the turning point from metaphysical to scientific philosophy—the 'Positive Philosophy' of Auguste Comte—remained three quarters of a century without being translated into the English language. This singular circumstance has led to some very peculiar results, and accounts for the totally false idea that the English-speaking world entertains with regard to Comte and his doctrines. Many suppose that he was a very bad, irreligious man. An eminent divine recently stated from the pulpit that 'Comte, the great French philosopher, taught that religion was only a phase of superstition that belonged to the childhood of the race and would be outgrown.' Interrogated as to where Comte taught this doctrine, he was unable to cite any work or passage. The fact is that Comte had a strong religious nature, and one of his aphorisms was that 'man is becoming more and more religious.'

Others, like Huxley (who does not seem to have read the 'Positive Philosophy'), see nothing of value in Comte's system. A common opinion is that it is a sort of utopia, and Comte's name is frequently associated with that of Fourier. Scarcely any one has the idea that he was a scientific man in the accepted sense of the expression, although he was by profession a mathematician.

The fact that Comte wrote another and later work, his 'Politique Positive,' in which he drew up a program of social regeneration and founded a cult, created the general impression that he was only a dreamer. His zealous followers from the standpoint of the cult saw to it that this work should be translated into English. There is no doubt that this did incalculable harm to Comte's entire system. For, in the first place, as M. Lévy-Bruhl clearly shows, it is impossible to understand the 'Politique Positive' without an acquaintance with the 'Philosophie Positive.' If Lévy-Bruhl had done nothing else than to dispel the illusion that the 'Politique Positive' was an after-thought, the product of a diseased mind, and a mere dream of a fanatic, it would have fully justified his writing this book. The few who have read the 'Philosophie Positive,' and especially those who have also read the five early papers written from 1819 to 1825, know already that the 'Politique Positive' was contemplated by Comte from the beginning, and was steadily kept in mind during all the patient years that it required to write the 'Philosophie Positive.' That work was to be simply the necessary preparation and scientific foundation for his final great con-

* *Carnegie Inst. Yearbook*, No. 1, pp. 241–266, 'Plan for a Biological Survey of the Palearctic Region,' by Leonhard Stejneger and Gerrit S. Miller, Jr.

struction. How many edifices have crumbled whose foundations have stood the test of time! But the superstructure was built upon that foundation, and every line of the 'Politique Positive' must be read with the 'Philosophie Positive' in full view, otherwise it is utterly incomprehensible. It is small wonder, therefore, that the readers of the English translation of the former, called Comte's 'Positive Polity,' could make nothing of it, and set Comte down as a dreamer or something worse.

Another marked consequence of the failure to translate the 'Philosophie' has naturally been a systematic plagiarism of Comte's ideas. Persons of a certain type, and they are common enough, finding a great body of original ideas in a work almost completely unknown, have made it their opportunity to pass themselves off as profound thinkers. A learned professor in one of the leading universities once came by invitation and read a paper on the classification of the sciences before one of the scientific societies of Washington. It proved to be simply a summary of the Comtian hierarchy, but Comte's name was not mentioned and the views were put forth as original with the speaker. There was only one member present who detected the plagiarism and felt it his duty to expose it. I have collected a large number of similar cases, many of which are amusing.

As a third effect of the same cause may be mentioned the manner in which Comte's ideas have influenced English thought. Accessible only to the highest types of mind, they found themselves reflected only from such high sources, and as such men usually have systems of their own, they strive to conceal the extent to which they are influenced by others. This has been notably the case with Comte's influence. In this day it is easy to see that it was very great in England. John Stuart Mill and George Henry Lewes were the most frank in acknowledging it, but it is now clear that Carlyle, Buckle and many others were profoundly affected. That Herbert Spencer recognized Comte's value to the scientific world no one now doubts, notwithstanding his vigorous disclaimers of discipleship. No one has maintained that he was a disciple. Indeed, the only disciples were those who accepted and strove to propagate his religion of humanity, and these usually cared very little for his scientific works; otherwise they would have had his 'Philosophie Positive' translated. That Spencer arranged his topics in substantially the Comtian order I have repeatedly shown,* but this does not imply discipleship, since it is the true order of nature. But Spencer could not have been ignorant of Comte's classification of the sciences. It had been before the world for thirty years before Spencer began the 'Synthetic Philosophy.' Others besides Mill had expounded it, and it was familiar to all the best minds. Spencer adopted both of Comte's new words 'sociology' and 'altruism,' and defended them with proper acknowledgments. Even his 'Social Statics' proved to be Comte's term, although Spencer thought it was Mill's. But his book by that name shows that he really had no idea of Comte's social statics or of social statics in any scientific sense.

Notwithstanding the handicap of a foreign language, Comte's fundamental doctrines have conquered the scientific mind, not only of England, but of the whole world. In conformity with the Scriptural saying, France was the last to recognize its own philosopher, but his day is now come. Streets are named for him, statues are erected to him, and his power has penetrated not only into academic but into legislative and administrative halls.

M. Lévy-Bruhl passes Comte's entire philosophy in review, but the treatment is somewhat uneven. The biological side is the least complete. It is true that Comte was not strong in biology, and accepted with certain reserves the doctrine of the fixity of species, as did nearly everybody in those pre-Darwinian days, but he was acquainted with Lamarck and believed in evolution and in the descent of man from an animal, even simian, ancestry. He worked out the doctrine of the interaction of the organism and the environment and carried it to a very advanced stage. He even foreshadowed the principle of natural selection, and it is remarkable that Lévy-Bruhl

* See SCIENCE, N. S. Vol. III., February 21, 1896, p. 294; 'Pure Sociology,' p. 69.

failed to point this out, especially as it had been pointed out by others, first in 1883 by the writer of this review,* and subsequently by Heinrich Waentig,† and also by his own countryman, M. Alfred Fouillée,‡ the passage being quoted in each case. It will bear quoting again. In the third volume of the 'Philosophie Positive,' which appeared in 1838, written, as he states in 1836, on page 392 he says:

> If we conceive all possible organisms to be successively placed, during a suitable time, in all imaginable mediums, the greater part of these organisms would of necessity finally disappear and leave only those surviving which could satisfy the general laws of this fundamental equilibrium; it is probable that, after a succession of analogous eliminations, the biological harmony must have established itself little by little upon our planet, where we still see it continually modifying itself in a similar manner.

A scarcely less remarkable passage occurs in the fourth volume (p. 443), which appeared in 1839.

Lévy-Bruhl's treatment of Comte's psychology is much more satisfactory. It may seem strange that he should devote a chapter to psychology, when Comte expressly repudiated the word. But he gives a simple explanation of all this. In Comte's day psychology meant 'the science of the soul reached through the introspective method.' 'It was the science founded by Cousin on the analysis of the ego,' as taught by the eclectic school of philosophers. "Comte, who opposes these philosophers, did not wish his theory of psychical phenomena, which differed from theirs, to be called by the same name." He said in 1828: "Some men, not recognizing the present and irrevocable direction of the human mind, have endeavored for ten years to transplant German metaphysics into our midst, and to constitute, under the name of *psychology*, a so-called science entirely independent of physiology." Comte refused to regard psychic phenomena as distinct from those of physiology, and, therefore, he included their study in biology, although he spoke of them as dealing with 'transcendental functions.' In fact, Comte thought that there might be a true science of *phrenology*, and tried to found that science, although, even in his day, as he himself knew and deplored, the process of prostituting that term had already begun, and soon after his death this had gone so far that the word, notwithstanding its perfect etymology and appropriateness, was wholly abandoned by scientific men, and the term psychology was resuscitated and adopted for the same science. But it is absurd to accuse Comte, as has been done, of lending any countenance to the vagaries of phrenology.

M. Lévy-Bruhl brings into clear relief the importance of sociology as a necessary part of Comte's scheme. When we remember that down to the year 1839 he had always called this science 'social physics,' first using this term in 1822, we can see very clearly what was in his mind. With him the characteristic of a true science was that its phenomena should conform to invariable laws. This he believed social phenomena to do. The name social physics was chosen to emphasize this view. It completed the series and supplied the final term that had always been lacking. 'We possess now,' he said, still at that early date, 'a celestial physics, a terrestrial physics, either mechanical or chemical, a vegetal physics and an animal physics; we still want one other and last one, social physics, in order that the system of knowledge of nature be complete.'

But he went farther and divided up the science on strict mechanical lines, founding both social statics and social dynamics. It would unduly extend this review to attempt to show how these two sciences were constituted, and the reader is referred to Lévy-Bruhl's fairly satisfactory presentation of the subject. But any mention of Comte's name almost requires the coupling of it with some reference to his celebrated law of the three stages (*trois états*) in the historical development of human thought, which constitutes the basis of his social dynamics. The attempt to maintain that this law was discovered and announced by Turgot, Condorcet, Burdin and

* 'Dynamic Sociology,' Vol. I., p. 119.

† 'Auguste Comte und seine Bedeutung für die Entwicklung der Socialwissenschaft,' Leipzig, 1894, p. 120.

‡ 'Le Mouvement Positiviste et la Conception Sociologique du Monde,' Paris, 1896, p. 101.

others is like ascribing the discovery of the principle of natural selection to Goethe, or Wells, or even to Comte himself, merely because in the writings of all these are to be found adumbrations of it. Comte it was who formulated the law and developed it at full length, devoting more than two volumes of the 'Positive Philosophy' to its elucidation, 'in all of which,' said John Stuart Mill, 'there is scarcely a sentence that does not add an idea.' Suffice it to say that it is a historical demonstration of the aphorism that ideas rule the world, and constitutes a complete philosophy of history based on the dictum of Leibnitz, that in all ages 'the present is full of the past and pregnant with the future.'

But this law has a still deeper significance, since whatever we may think of the theological and metaphysical stages of history, the positive stage is the age of scientific thought. The full characterization of this stage is the essence of the positive philosophy. As George Henry Lewes said, 'positive thinkers may be counted by thousands, but no one before Comte had a glimpse of the positive philosophy.' Now, to add sociology to the 'hierarchy' of the sciences was simply to complete the scheme of the 'Positive Philosophy.' Without it the scheme was truncated. But sociology was more than any of the other sciences. It was in a certain sense the science of the sciences, since it presupposed and embraced all the rest. An acquaintance with the others was necessary to it, because it was a synthesis of them all, and dealt besides with social phenomena, with which none of the others had anything to do.

So far as ethics is concerned, M. Lévy-Bruhl clearly shows that it is with Comte simply an aspect of sociology. Comte's ethics is not a moralizing, or a treatise on duty or on right and wrong, but a discussion of the origin of ethical ideas, growing out of the, as he claimed, spontaneous sociability of men—in a word, it is social ethics.

We need not follow our author farther and point out the 'positive transpositions,' or revaluations, that the positive philosophy has wrought in the ideas that prevailed before Comte's day. It is sufficient to have shown that M. Lévy-Bruhl has set him in a just light before the world as a great organizing genius. His knowledge relatively to the time he lived was, like that of Spencer for his time, encyclopedic. Different as the systems of these two philosophers are, they each have practically the same relation to their respective dates and times. Comte was the philosopher of the first half, as Spencer was of the second half, of the nineteenth century. Comte's weight turned the scale in favor of the scientific method, and inaugurated a positive, which is the same as a scientific, *Weltanschauung*, destined to banish the hitherto prevailing theological and metaphysical conceptions of the universe. As Comte's works are more fully studied it is found that they constitute a vast storehouse of ideas. When a supposed new thought is put forth by some modern writer nothing is more common than to find that Comte had given clear expression to it more than half a century earlier. Much of the contemporary sociology consists in the rediscovery of the truths that Comte reached and fully set forth, and sociologists are just beginning to learn that they must go back to Comte as certainly as the metaphysicians must go back to Kant.

M. Lévy-Bruhl, in citing Comte's 'Philosophie Positive,' has used the fifth edition, 1892, which, unfortunately, is not uniform with all previous editions, and this makes the verification of passages somewhat difficult for those who can only consult the earlier and better known editions. The English translation bears the marks of 'business enterprise.' The short note by Mr. Frederic Harrison is magnified on the title-page into an 'introduction.' It is matter for regret that Mr. Harrison did not really contribute an introduction. On the other hand, the name of the translator, Kathleen de Beaumont-Klein, who contributes an excellent preface, does not appear on the title-page.

The translation is in the main good, but it is easy to find what are called 'gallicisms,' such as using the word *ignore* in the French sense of not to know, writing *movement* for *motion*, *experience* for *experiment*, *precious* for *valuable*, *conscience* for *consciousness*, etc. There certainly was no excuse in an English

translation for citing all German works by their French titles. LESTER F. WARD.

WASHINGTON, D. C.

Light Waves and Their Uses. By A. A. MICHELSON. Decennial Publications of the University of Chicago. Second Series, Volume III. University of Chicago Press, 1903.

The 'uses' with which this book is concerned are altogether those with which the author's name is so intimately associated; that is, the applications of interference methods whereby light waves are made the tools and units of measurements for physical and astronomical investigation. The Michelson form of interferometer, which has tremendously increased the applicability of this method, was invented as a means of attack upon one important problem which is here treated briefly —the well-known Michelson-Morley ether-drift experiment—still the subject of study, both experimental and theoretical.

An introductory chapter on wave motion and the general phenomena of interference serves to prepare the reader for the development of the interferometer principle, by which is meant the use of a plane reflecting and transmitting (glass) surface to split a beam of light into two, which are subsequently recombined, to produce interference fringes. The quantity directly measured is either a movement or shift of these fringes, or a change in their distinctness or 'visibility,' produced by changes in the relative retardation of the two beams between the points of separation and recombination. By this means changes in the relative retardation, which may in a particular case be produced by changes in position of a plane reflecting surface, can be measured with extreme accuracy. Again, the change in relative retardation may be produced by changes in the index of refraction of the medium through which one beam passes, or motion of the medium, or by the introduction of transparent films—and the corresponding shift of the fringes affords an exceedingly accurate means of measuring these changes.

Some of the many special cases in which this method has been applied are dealt with in succeeding chapters; for example, the measurement of angles and distances, the study of spectrum lines and close groups of lines, the effect of a magnetic field on light-emission, the determination of the angular magnitude and 'structure' of stars, and the fundamental, but less fascinating, matter of the use of light waves as standards of length—*i. e.*, the evaluation of the meter in terms of the wave-lengths of the red, green and blue radiations of cadmium.

The book is avowedly popular, being a reprint of Lowell Institute lectures, and the lecture style is retained throughout; nevertheless, it is to be feared that without the aid of experimental demonstrations, for which the good illustrations are hardly an equivalent, the 'general reader' would be rather overtaxed by some of the chapters. However, from the other standpoint of the preface, the book as a résumé in untechnical form, of important researches which have occupied Professor Michelson for the past twenty years, will be of great value, not only to scientists who have not read the original papers, but to many who have. C. E. M.

Index to the Literature of the Spectroscope (1887 to 1900, both inclusive). By ALFRED TUCKERMAN. Smithsonian Miscellaneous Collections, 1902.

This index forms a continuation of a previous volume by the same author, which dealt with the literature up to 1887, and continues the subject up to the time when the work was taken over by the International Committee for Indexing Scientific Literature. The first half of the book is taken up with the author index, alphabetically arranged, of which the chief characteristics should be accuracy and completeness. Concerning the former a short examination suffices to detect a fairly large number of misprints, mostly trivial, besides a few cases of confusion of names, and one erroneous reference. Again, while absolute completeness is too much to ask for, there are omissions here which do not seem based on a fair estimate of the relative importance of various papers.

The second half of the volume contains the

same titles arranged according to subject, and here the matter of judicious choice of main and subheading, the distribution of titles among them, and cross-referencing, are of especial importance. In some cases, as, for instance, the heading 'Electric spectra,' too little subdivision has been made, while in others, for example, infra-red work, too many and not sufficiently distinct subheadings have been introduced. Cross-references and a list of the subdivisions of the subject index would be a great addition; and the more frequent insertion, as is done in some cases under 'absorption spectra,' of a few words of explanation as to the scope and character of the work would add greatly to the usefulness of this part of the volume.

In spite of these faults, however, and in spite of the fact that Kayser's 'Handbuch' will doubtless contain more references, this bibliography should be of considerable value.

C. E. M.

SCIENTIFIC JOURNALS AND ARTICLES.

The American Naturalist for December, 1903, presents the third of the series of articles on 'Adaptations to Aquatic, Arboreal, Fossorial and Cursorial Habits in Mammals,' the present being by H. W. Shimer on 'Fossorial Adaptations.' These are fewer in number than those for other modes of life, but among them the writer fails to include the use of the tail as a tactile organ, making the mistake of supposing it to be 'a useless appendage.' W. Patten gives a valuable paper 'On the Structure of the Pteraspidæ and Cephalaspidæ' with the purpose of strengthening his theory on the genetic relationship between the vertebrata and arthropoda, and James G. Needham describes 'An Out-Door Equipment for College Work in Biology.' Unluckily, all colleges are not so well situated as that of Lake Forest. W. McM. Woodworth has a most interesting 'Preliminary Report on the Palvlo Worm of Samoa, *Eunice viridis* (Gray.)' W. E. Ritter gives 'Further Notes on the Habits of *Autodax lugubris*,' including the important information that this species breeds in holes in trees. The concluding paper, by Wilmatte P. Cockerell, describes 'A Trip to the Truchas Peaks, New Mexico.' The number contains the 'Quarterly Record of Gifts, Appointments, Retirements and Deaths.'

The American Museum Journal commences its fourth volume with the January number; it contains much information as to new exhibits, including notes on 'The Behavior of the Minerals and Gems of the Morgan Collections toward Radium and Other Sources of Light,' 'The Long-tailed Japanese Fowls,' 'The Draught Horse in Action,' 'Extraordinary Ants' and 'The Exhibit of Chuckchee Clothing.' The skeleton of the great Percheron, mounted by S. H. Chubb, is the best mounted skeleton we have ever seen and shows what may be done in this direction. The Supplement, Guide Leaflet No. 13, is an illustrated General Guide to the American Museum of Natural History.

SOCIETIES AND ACADEMIES.

ANTHROPOLOGICAL SOCIETY OF WASHINGTON.

THE twenty-fifth annual meeting was held January 12. The following officers were elected:

President—Dr. D. S. Lamb.
General Secretary—Walter Hough.
Curator—Mrs. Marianna P. Seaman.
Treasurer—P. B. Pierce.
Councilors—Dr. George M. Kober, J. D. McGuire and Dr. J. Walter Fewkes.

The 254th meeting was held January 26. Dr. W J McGee reported progress of the various expeditions to secure examples of interesting tribes for the Louisiana Purchase Exposition. It is intended to have at St. Louis families of Central African pygmies, Tehuelches of Patagonia, and Ainos of Hokkaido, and members of tribes of the United States engaged in ancient industries. A model school for Indians will be another attractive feature.

The first paper was by Professor W. H. Holmes, the title, 'One of the Great Stone Buildings of Yucatan.' The paper was illustrated by a superb model made for exhibition at St. Louis. Professor Holmes said that the architecture of the natives of America is not

well understood, and the subject has received less attention than it deserves. There is, however, a widespread interest in the more noteworthy products of the skill of the aboriginal architect and builder, and for this reason five examples of the great buildings of Mexico have been selected and models prepared. The building described is the largest in Uxmal, and is 320 feet long, 40 wide and 20 high. The walls are of rubble cement and earth faced with limestone, the front wall 4 feet and the back wall 9 feet thick. The remarkable feature of this building is the cornice, of which there is 740 feet, with 10,000 stones set in it. Masks numbering 180, requiring 5,000 stones, meander over the cornice, and at intervals are placed seated figures bearing elaborate head dresses. The building was a residential one, and was the first of a projected group around a square. The paper was further illustrated with diagrams and detail models.

Professor L. S. Rowe, of the University of Pennsylvania, presented a brief communication, entitled 'The Work of the American Academy of Political and Social Science.' Professor Rowe said that there is serious danger that political science will become a mere dogma, and for this reason anthropology should come to its aid, mainly by defining the origin of social relations. To this end he bespoke the good offices of anthropologists in the field of political and social science.

Dr. McGee said, in discussing Professor Rowe's communication, that anthropology and political science are closely related, and that the appreciation of the aid of anthropology is gratifying. The most advanced views are upheld in the Anthropological Society of Washington. In this society there are sections each devoted to an aspect of the science of man. One is sociology, which embraces the relations of man as groups. Political science represents but one aspect of the same object that social science has to deal with. Political science has a narrower field, and deals with only one face of the great diamond.

Dr. Alës Hrdlicka read a paper entitled 'The Indians of Sonora, Mexico.' The paper was illustrated with lantern views of the people and scenery. Dr. Hrdlicka confined his remarks to the Yaki and Opata, giving a historic account and showing their geographical distribution. The Yaki are a virile tribe, and are not declining, while the Opata are being amalgamated. Dr. Hrdlicka gave an interesting account of these tribes, of which there is so little scientific observation. Numerous slides illustrated the paper which will appear in extenso in the forthcoming *American Anthropologist*.

Walter Hough,
General Secretary.

CLEMSON COLLEGE SCIENCE CLUB.

The club held its regular monthly meeting on December 18, 1903. Professor C. C. Newman gave a paper entitled 'Notes on Pecan Culture.' The speaker brought out the fact that the pecan grows wild over quite a large extent of territory in the United States. Methods of propagation were described, the fact being brought out that the usual method of planting seed for the production of seedlings is unsatisfactory, since these latter do not come true to seed. The high price at which the nuts are sold for seed was mentioned in passing. The only satisfactory method of propagation is by grafting. The speaker, by means of a number of specimens, illustrated in detail the different methods of grafting used. In closing, the possibilities of pecan culture on a commercial scale in the south and, especially South Carolina, were pointed out. Quite a large amount of interesting illustrative material was used by the speaker who gave this paper.

Professor J. Volney Lewis gave a paper entitled 'Notes on the Physiographic Development of the Rocky Mountains.' The author prefaced his paper with an outline of the leading events in the development of the Rocky Mountains in their relations to other portions of the continent. Some account was then given of observations made in the summit region of the Rockies between the North Platte valley in Wyoming and the canyon of the Arkansas in Colorado, in connection with the work of the U. S. Geological Survey in the Encampment region during the summer of 1902. The closely folded and faulted pre-

Cambrian rocks of the Encampment region, with east-west axes, were reduced to a peneplain at a time as yet undetermined, and the warping of this old surface by post-Mesozoic disturbances produced the arches of the present mountains, with their axes north and south. This peneplain is recognizable on the continental divide and the long spurs and parallel ranges to the east and west in the Encampment region. The sedimentary strata occupying the flanks of the mountains and valleys and 'parks' of this region doubtless once covered most, if not all, of the peneplain, but it has been laid bare over large areas of higher ground by subsequent denudation and more or less deeply incised by the streams. Brief reference was made to the dissection of the floors of some of the 'parks' by the streams draining them; to the systems of terraces occurring along nearly all the larger streams of the region and the diverse hypotheses accounting for their origin, and to the minor glacial phenomena. The paper was illustrated by a considerable number of lantern slides and maps.

F. S. SHIVER,
Secretary.

DISCUSSION AND CORRESPONDENCE.

CONVOCATION WEEK.

TO THE EDITOR OF SCIENCE: I heartily agree with the views expressed in your editorial of January 8 in regard to the affiliation of the various national scientific societies. If scientific men in this country are to exercise the influence they ought to exercise in educational matters; if they are to influence legislative action when it concerns scientific work; if they are to hold the place in the country which is due to them and their work, they must come together; they must learn to know each other and to act as a unit. It seems to me that it is very important to bring the scientific bodies together at least once a year. The American Association for the Advancement of Science has taken the initiative because it is the largest of all of the scientific societies. Its policy has been to hold a meeting during convocation week and invite the other scientific societies to meet at the same time and at the same place. The greatest freedom has been granted these bodies, and all the privileges which the large membership of the American Association for the Advancement of Science secures have been granted to them. There seems to be a feeling on the part of some of the societies, however, that the large association is trying to influence their action and force them into affiliation with it. I am confident that the only desire of the American Association is to bring the scientific men together for the good of all.

I would suggest that each national scientific society be asked to send a representative to a meeting to be held at some central point before next summer, where this whole question can be discussed in all of its bearings, and see whether it is not possible to arrange for meetings of all of the societies at the same time and place once each year or at other stated intervals. Such a committee could discuss the advantages and disadvantages of such gatherings, the influence which could be brought to bear upon scientific education and research and many other matters which would naturally suggest themselves. I believe that SCIENCE might take the initiative and request the various scientific societies to send representatives to such a meeting.

CHARLES S. HOWE.
CASE SCHOOL OF APPLIED SCIENCE.

As the editor of SCIENCE has pointed out, the advantages of the winter meetings of the American Association and of its affiliated societies are evident, and they certainly are desirable for those who can afford to attend them. The council, it is to be presumed, would be glad, however, to hear and to consider objections. Those of us who live on the Pacific slope have some that are peculiarly our own.

1. In order to attend a winter meeting we are obliged to rush off at the beginning of our very short Christmas vacation, and to spend a large part of that vacation crossing and recrossing the continent.
2. The trip is one of several thousand miles that requires from six to ten days on trains.
3. For this long trip we are unable to obtain

any considerable reduction of railway rates.

4. The season has certain objections for some of us who would like a few days of quiet with our families during the Christmas holidays.

The second and third objections may seem to hold also against the summer meetings of the association. But as the summer meetings fall in the long vacations a trip to the east is generally utilized for other purposes as well as for attending the scientific meetings, while the short Christmas vacation does not leave us time for anything else.

We of the Pacific slope, however, fully realize that this question should be settled with a view to the interests of the majority of the scientific men of the country, and that the majority lives east of the Rocky Mountains. We hope, though, that it will not be forgotten that the association has always tried to do a little missionary work whenever it has been possible. J. C. Branner.

To the Editor of Science: Replying to your request for an expression of opinion relative to scientific organization in this country, I would say that the reorganization of the medical profession seems to me to offer an example which we might well follow.

Let the present American Association for the Advancement of Science be taken as basis, forming the national organization; let the present council be supplanted by a house of delegates, composed of representatives from each state society, the number being in proportion to the state membership; let all business affairs, questions of policy, etc., be transacted by this house of delegates; let the national association hold its meetings in convocation week, and be divided into as many sections as seems necessary or desirable for the purpose of presenting papers, but let it be divided into state associations for the purpose of representation and government. The national association can prepare a model constitution for the use of the state and local associations and have such constitution cast so that only the state or local names need be inserted, thus saving considerable expense, work and worry.

The state associations should hold independent meetings or two or three adjoining states could unite for joint meetings, preferably in summer. A division into sections could obtain for presentation of papers, but the business affairs could best be conducted by a state house of delegates, composed of delegates from the local associations. Such meetings would doubtless be attended by many college assistants, college students and especially high school teachers who can not afford to attend the national meeting and who might feel lost if they did attend.

Local associations could be formed in any community in which a given number of scientific workers could be organized. It is but natural that the universities and colleges would form the centers of the local societies and by interesting all senior students in the local societies, the national association would eventually practically represent a national alumni organization. At least all scientific students in colleges and universities would, upon graduation, enter the post-graduate national organization. In places like Boston, New York, Washington, Chicago, etc., there would be a differentiation into sections, according to subjects represented and members enrolled.

Such an organization would unite all American scientists into one strong body, which upon occasion would be an important factor in national or state legislative and other public matters. It would give every member of a local scientific society representation in the national organization. It would decrease expenses in local and state societies, and thus decrease the number and amount of annual dues, which are already too large. It might enable us gradually to assume control of and responsibility for technical publications. It might result in the union of several existing publications with the plan of distribution by signatures, so that it would be possible for a zoologist to subscribe for all of the zoological papers issued without his purchasing four times as many papers on other subjects, and the same advantage would accrue to the botanist, the chemist, the mathematician, etc. And to the great delight and relief of the bibliographer, such a plan would probably re-

sult in the early death and future prevention of the numerous struggling, ephemeral publications issued by various local organizations.

There is nothing original in the suggestions made above. The plan has been inaugurated by the medical fraternity and its good results are already evident. Would it not be wise for the scientific organizations to consider the same plan seriously, even if it should involve the demise of several existing organizations?

CH. WARDELL STILES.

WASHINGTON, D. C.

TO THE EDITOR OF SCIENCE: The problems connected with convocation week may be approached from the point of view either of that which is in the abstract most desirable or of that which is best adapted to present conditions. The latter question can not be treated properly without some consideration of the former, but it is idle to confine our attention to the ideal.

Few men of science can fail to recognize the enormous advantages resulting from the unification of all the scientific interests of the country under a single organization. The main question seems to be whether it is practicable to effect such a unification without the sacrifice of other and more important interests.

Objection is made on the part of the technical societies to any form of affiliation which would tend to degrade the professional standing of the membership of the societies. The objection certainly is a valid one, and reference to the editorial of January 8 will show that no such effect of affiliation on the professional societies is necessary or desired.

The objection to joint meetings growing out of the great extent of territory covered by the national organization and the resultant expense in time and money involved in reaching distant cities is common to any form of national society, however limited in scope, and may be met practically in a variety of ways. It certainly is not greater with the affiliated societies than with those entirely separate.

Another difficulty not so easily disposed of grows out of the crowding together of many programs in the short space of one week and the interruptions due to the numerous public lectures and social events of general interest. This situation presents a problem of the first magnitude and one better solved by a process of natural growth in experience than by academic discussion.

We have already had experience with three chief methods in the conduct of our mid-winter meetings. (1) Each society has met independently, with no attempt at correlation with any other organization. This was the method until very recently, with the result, perhaps, that three societies with which one might wish to keep in touch were meeting simultaneously in three widely separate cities, a situation so intolerable as to have provoked the agitation for convocation week.

(2) A second method is for the independent societies by mutual agreement to meet at one time in the same city. In the biological sciences we had a practical illustration of this at the last Washington meeting, where several sessions were held simultaneously offering programs covering substantially the same field. This situation was far from satisfactory to any of the membership, some declaring that it were better to return to the former plan of wholly separate meetings. Others considered that the resultant social advantages more than compensated for the distressing conflict of desires by which one was torn during the hours devoted to the reading of papers.

(3) The third method is for all of the societies to meet together and for those dealing with related subjects to combine their programs so as to have but one series of papers on the same subject running at one time. An inordinate length of program on any subject may be obviated by a further topical division of the program into smaller sections for all or part of the time. To be sure, it is a very difficult matter sometimes to find a natural line of cleavage in a program, but a separation of papers in accordance with any principle, however defective, is better than a separation based on no principle save the accidents of membership in the several societies; and if the programs are published in advance and are as closely followed as possible and espe-

cially if the paper in course of reading and the paper next to be read are posted outside each section room during the sessions, then it will be possible for any one at least to choose intelligently between papers appearing simultaneously, instead of leaving this largely to chance, as was too often the case at Washington.

That some such plan is not impracticable is shown by the very recent history at the St. Louis meetings, as illustrated by the program in zoology. Section F and the Central Branch of the American Society of Zoologists, by previous arrangement of their executive committees (these societies, by the way, not even being affiliated), combined for program purposes. This arrangement provided that all of the papers submitted and approved by the respective program committees should be considered by a joint program committee just before the meeting and that all papers approved by this joint committee should be pooled and arranged in a single program classified by subjects, with the proviso that if this made too long a program they should be divided into two groups on the basis of subject matter, not on the basis of the societies presenting them, and that simultaneous sessions should be held for such time as might be necessary. Conference with the executive officers of both societies after the meeting shows that this plan worked out to the satisfaction of all concerned with no friction or other unpleasant features save for some inconvenience to the secretaries growing out of a tardy beginning of the negotiations. A critical examination of the joint program shows, as it happens, that the two societies represented were very evenly balanced, both in number of papers submitted and in their scientific worth.

The plan here outlined is not recommended as an ideal, nor even a practicable, solution of the problem for every case, but it is mentioned as another illustration of the fact that the problem can be solved in a concrete case. Doubtless a different solution would have to be worked out for some other case.

In view of this history, the present writer, who is a loyal member of both Section F and the American Society of Zoologists, fails to see any reason why the societies should not have the advantages which would accrue to both by such an affiliation as was referred to above, uniting their forces for certain executive functions of national import, but leaving each free to work out the details of its organization in its own way. Either branch of the professional society would be able to meet independently at any time, but when the American Association meets in its region it should, as a rule, secure the advantages of joint sessions.

For my part, to be perfectly frank, I do not go to the national meetings primarily to hear papers read, much as I enjoy that feature, for these can generally be studied at leisure afterwards. As a comparative neurologist I, of course, want to get the first word of every advance movement in my specialty in this way; but I prize not less highly the opportunity possible only in a gathering of many different societies, of meeting my colleagues in related fields, by attending the Physiologists' and Anatomists' smokers, the Naturalists' dinner, and in other ways keeping in closer personal touch with the men who are doing the work in related departments of research on which I must so largely build. This is for me one of the most important gains that I win at the national meetings and one that can not be fully realized in any meeting of a single professional society, no matter how high the scientific standard or how wide a territory may be represented.

To secure this gain I, for one, am willing to make considerable sacrifices, though, as indicated above, these sacrifices do not necessarily include any loss in the efficiency or interest of the programs of convocation week. My own feeling is that in the final adjustment we shall probably come to a summer meeting of the association, with field excursions made prominent, some of the sections perhaps meeting in different places, and a convocation meeting in the winter devoted mainly to the reading of technical papers, with a due proportion of the time devoted to public discussions and lectures on themes of general scientific interest and to social intercourse. There should

be no difficulty in maintaining in these programs a scientific standard of papers read as high as that now presented by the professional societies. Indeed, there would be no impropriety, particularly in the case of any section which holds summer meetings, in relegating the reading of technical papers at the convocation meeting to the affiliated professional societies, the section, if possible, offering, in addition to the vice-presidential address, one or more set papers or discussions of more general interest.

The solution when finally wrought out will no doubt come by a process of evolution rather than by revolution, and the present trend is clearly in the direction of reserving the time of convocation week very largely for the reading of papers by the technical societies. Any attempt to force these societies into the summer months is foredoomed to failure.

C. JUDSON HERRICK.

THE CASE OF WILLIAM J. LONG.

TO THE EDITOR OF SCIENCE: The criticism of William J. Long's published observations on the habits of animals inaugurated so vigorously by John Burroughs in *The Atlantic Monthly* for March, 1903, and continued no less forcibly by William Morton Wheeler in your issue of February 26 arouses no little interest in the personality and methods of a writer whose work has met with so unfavorable a reception by naturalists.

Are we to believe the accusation that the author in question, to put the matter squarely, is a 'liar,' or have we in Mr. Long a naturalist whose powers of observation, discrimination and interpretation are so far beyond those of any other student of nature, living or dead, that he is in effect a Galileo among animal psychologists?

It can not be denied that Mr. Long, in spite of his youth, has placed upon record more remarkable statements regarding the behavior of the birds and mammals of New England and New Brunswick than can be found in all the authoritative literature pertaining to the animals of this region.

The story of the crows and their game with a china ring; of the kingfisher that stocked an isolated pool with fish in order that it might easily teach its young the art of fishing; of the partridge that repeatedly drummed a roll-call for the two missing members of a brood of eleven; of the red squirrel with cheek-pouches; of the porcupine that coiled in a ball before rolling down hill; of the loon that hatched its eggs, not by sitting on them, but by gathering them close to her side with her wing; of the woodcock that placed its broken leg in a plaster cast; of the ducks that have learned to drown salt-water mussels in fresh-water pools; of the great blue heron that scattered a pollywog in fragments on the water as bait to draw fish within spearing distance: these and many other equally remarkable observations and experiences are recounted with a circumstantial detail that carries conviction to all but the informed.

Indeed, one has not to read far in any of the half a dozen or more volumes which Mr. Long has produced to discover some more or less remarkable description of the actions of animals.

The nature of their contents and their undeniable literary merit furnish abundant reason, therefore, why Mr. Long's works should claim not only the attention of naturalists, but of the public generally, and again it may be asked, is the unsparing criticism to which they have been subjected warranted?

An apparently satisfactory reply to this query is furnished by a defense of Mr. Long published in the Boston *Evening Transcript*, March 7, 1903, and by Mr. Long himself. The writer of the *Transcript* communication seems to have been acquainted with Mr. Long at Andover Theological Seminary. After saying that Mr. Long prepared himself for college by 'solitary' study and that in entering the sophomore class he had not experienced that year in an undergraduate's life 'when a young man learns to take himself for very little,' he continues:

> From both these circumstances it comes about that we have here a man easily tempted to overrate his personal knowledge, a man tempted to superficiality, a man likely to draw rash and ill-considered conclusions. It is also to be remembered that Mr. Long is of Irish extraction—inflammable, poetic and volatile in temperament.

No matter how great his learning up to a certain point, he is continually in peril of making irrational ventures beyond that point, lured forward by pure imagination. This has come out in his preaching. Eccentricities and extremely radical outbursts have had a disturbing effect upon his audiences and have limited his success. It is just this sort of thing that has made him the prey of Mr. Burroughs. I well remember an utterance of Mr. Long's in a class-room in Andover when he took issue with the professor, beginning by saying: 'I always love to think'—to which the professor replied, 'Mr. Long, we are not concerned with what we love to think, but with what we ought to think.' Here was the whole situation in a nutshell. What he has loved to think and not what he ought to think has colored whatever has met his eyes in the theological as well as the biological world.

Honest, absolutely honest, and yet not quite telling the truth—that is a seeming paradox, but a real paradox only as many a poetic temperament is itself a paradox, and any poetic temperament, any temperament to which imagination is all but reality, and to which the thing loved and, therefore, the thing sought, is by a natural consequence the thing believed—any such temperament will prepare bitter grief for itself when it enters the world of natural science. Scientists have always to guard against the personal equation. This is well illustrated in the disappearance from scientific use of the pencil sketch and its replacement by the photograph. Let me draw the strata in yonder rocks, and nine chances in ten I shall unconsciously draw into them the theory which I intend them to illustrate. The camera, on the other hand, tells no lies, and very plainly Mr. Long is some other thing than a camera. His finished product is art, not science; it is the forest plus Mr. Long; it is the woodland folk introduced, interpreted, beloved—I had almost said at the first, created by Mr. Long. And I wonder whether, after all, Mr. Burroughs is not equally writing his own delightful personality into his own charming pages. The world-wide difference comes in at one point only. Mr. Burroughs is temperamentally fitted to interpret nature through the forms of literature; Mr. Long is not so fitted.

Evidently the author of these illuminating paragraphs knew whereof he wrote, and his delineation of Mr. Long's peculiar characteristics appears to explain satisfactorily that gentleman's ability to make more startling discoveries in one short lifetime than have fallen to the lot of naturalists in preceding centuries. Additional light is thrown on Mr. Long's methods and their results by his confession that he has 'never studied nature consciously, but only loved it,' and has found out many of its 'ways long ago, guided solely by a boy's instinct' ('Ways of Wood Folk,' preface); while the dedication of 'Fowls of the Air' is a surprising avowal of its author's point of view. It reads:

> To the Teachers of America who are striving to make nature study more vital and attractive by revealing a vast realm of nature outside the realm of Science, and a world of ideas above and beyond the world of facts, these studies from nature are dedicated.

To the naturalist further comment will be unnecessary, but it doubtless will be inquired why make all this disturbance about one of scores of inaccurate producers of so-called 'nature' books? Chiefly, it may be replied, because of the magnitude of Mr. Long's offenses, of the audience which he has won through his marked literary gifts in descriptive writing, and of the prominence injudicious criticism has brought him.

In a well-meant but somewhat ill-considered attempt to stamp out the fire, Mr. Burroughs merely scattered it. From an insignificant smudge, it has become a roaring blaze and its sparks are kindling throughout the land.

It requires the briefest consideration of the fact that tens of thousands of Mr. Long's books have been sold for supplementary reading in the schools—where, judged only by their literary charm, they are almost uniformly commended by teachers—to realize their far-reaching influence for evil. As I write, a prominent educational journal is received containing a review of Mr. Long's latest book, which the reviewer says, is 'by one who knows whereof he speaks, and who has studied so carefully and lovingly that he will make revelations that will hold us breathless.' This is a fair indication of the esteem in which Mr. Long and his works are held by the average teacher.

Is it not, then, the duty of naturalists to enlighten the general public, and especially those entrusted with the education of children,

in regard to the real character of Mr. Long's efforts to reveal 'a vast realm of nature outside the realm of science' in 'ideas above and beyond the world of facts'?

FRANK M. CHAPMAN.
AMERICAN MUSEUM OF NATURAL HISTORY.

THE METRIC SYSTEM.

TO THE EDITOR OF SCIENCE: It is now years since the metric system has been authorized and permitted in this country and yet very little progress has been made in its practical introduction. We still labor with the old system. We can never tell in statistics or contracts what a ton of coal means (long or short) unless it is explicitly stated. And so in water analyses, they are stated in three or four different ways, so that it is hard to compare them. Even if we know they are in grains per gallon, it remains to be determined whether the gallon is imperial or U. S.

Allow me to suggest a method of introducing the metric system which might meet much less friction and meet all practical purposes.

The proposed legislation would be as follows:

1. On and after January 1, 1906, the U. S. standard ton shall be the metric ton, which shall contain 2,000 U. S. standard pounds, each of which shall contain 16 U. S. standard ounces. No other ounce, pound or ton weights, or weights purporting to be fractions or multiples thereof, shall be used under penalty.

2. The U. S. standard pint shall be the volume of one U. S. standard pound of pure water under certain conditions of temperature and pressure, and shall be equivalent to one-half liter. A quart shall be two such pints, and a gallon four such quarts, and a peck eight such quarts, and a bushel thirty-two such quarts, and no other measure of volume shall be permitted, the distinction between fluid and dry measure being abolished.

3.* The U. S. standard foot shall be the length of the edge of a cube which shall contain 1,000 U. S. standard ounces of water under certain conditions of temperature and pressure, *i. e.*, 62.5 U. S. standard pints. The popular use of the terms would not need be changed *at all*, and the actual change of units would be so slight (ten per cent. or less) that it would not popularly be noticed, as may appear from the following table:

United States Standard.						Metric.
.985	present ton	=	1	proposed ton	=	1 metric ton.
1.102	" pounds	=	1	" pound	=	½ Kilo., German 'Pfund.'
1.102	" ounces	=	1	" ounce	=	1/32 Kilo. = 31¼ grams.
1.05671	" liquid pints	=	1	" pint	=	½ liter.
.9081	" dry pint	=	1	" quart	=	1 liter
.9081	" " bushel	=	1	" bushel	=	32 liters.
1.05671	" liquid gallons	=	1	" gallon	=	4 "
.984	" foot	=	1	" foot	=	25 ∛2 cm. / 315 cm.

Moreover, the old proverb, 'A pint is a pound the world around,' will be strictly true, and in water analyses a nickel's weight in a pint will be the same as an ounce per cubic foot and, specific gravity apart, the same as parts per thousand.

Especially in ending the long wrangle over various tons, I think the proposed changes would be decided improvements, and the differences between wet and dry measure should be abandoned.

ALFRED C. LANE.

SEX DETERMINATION IN BEES AND ANTS.

IN SCIENCE for December 25, 1903, Professor W. M. Wheeler characterizes as lacking in critical caution and 'apodictic' the statement that 'the egg of the bee, if unfertilized, invariably develops into a male, but if fertilized into a female.' If Wheeler's objection is directed merely against the form of this statement and not against its general content, if he desires merely the eradication of the word

* This is not so essential to the scheme.

invariably and would see it replaced with *so far as observed,* I am quite ready to grant all that he desires. It scarcely requires explicit statement here that *all* conclusions of inductive science must be so qualified.

But if, as seems possible, Wheeler's objection extends farther and he would have us understand that the generalization made is supported by insufficient or uncritical observation, I would join issue with him sharply. Dzierzon's theory did not grow out of idle speculation, as a casual reading of Wheeler's article might lead one to suppose; it was the outgrowth of much careful observation and thought on the part of a keen-eyed bee-keeper. It won its way to general recognition in the face of bitter opposition and has successfully withstood for half a century repeated assaults from various sources, scientific and otherwise. A brief summary of the evidence on which it rests may not be out of place here.

1. Dzierzon showed more than fifty years ago* that mating of the queen-bee (the egg-laying female of the hive) does not take place within the hive, but high up in the air. It takes place, if at all, before the queen has begun to lay eggs, and occurs but once in the lifetime of the queen, viz., in what is called her 'nuptial flight.' For this flight she issues from the hive on a bright still day. Her seminal receptacle then contains only a thin watery fluid, as Dzierzon and his coworker, Berlepsch, found by dissection. When she returns, the seminal receptacle is swollen and opaque, crowded with spermatozoa. This observation we have on the added authority of von Siebold, who made microscopical examination of the seminal receptacle of a queen captured as she returned from the nuptial flight.

2. If for any reason the queen is unable to take the nuptial flight, as because she has crippled or defective wings, or because her wings have been artificially removed, she is not prevented thereby from laying eggs capable of development, but from such (unfertilized) eggs develop only bees of the male sex (drones). This conclusion, the outcome of repeated observations made by Dzierzon, Berlepsch and Bessels, is further supported by an experiment made by Berlepsch. He induced a hive of his bees to rear queens late in the season (near the end of September), after the drones had disappeared. One of these queens, which was wintered over, produced in the following spring some 1,500 cells of drone brood in worker cells. A dissection of this queen made by Leuckart showed that she really was, as expected, unimpregnated.

3. Worker bees, which are only imperfectly developed females, sometimes lay eggs capable of development. This frequently occurs after a hive has lost its queen. From such 'worker' eggs develop only male offspring. Dissections of egg-laying workers, which were made by Leuckart, revealed no seminal receptacle, hence the eggs of such animals can not have been fertilized.

4. Old queens, which possibly have exhausted or lost control of the supply of spermatozoa received at the nuptial flight, sometimes produce only drone offspring (in worker as well as in drone cells). An old but fruitful queen, which had been producing offspring of both sexes, was accidentally crushed toward the tip of the abdomen by Berlepsch, so seriously that he thought her dead, but she revived after about an hour and was replaced in the hive. All the eggs which she subsequently produced (and they numbered thousands) developed into drones. This case is, with a good show of reason, explained on the hypothesis that the genital organs of the queen were so injured by the accident that thereafter none of her eggs could be fertilized.

5. Queens which have mated in normal fashion subsequently lay eggs some of which are fertilized, others unfertilized. The fertilized eggs are deposited ordinarily only in the small worker cells or the very large queen cells, and they develop into females. The unfertilized eggs are deposited ordinarily only in the drone cells, and they develop into males.

* The original papers of Dzierzon were published in a bee journal now not generally accessible, *Die Bienenzeitung,* but extensive quotations from them are contained in the classical paper of von Siebold, 'Wahre Parthenogenesis bei Schmetterlingen und Bienen,' Engelmann, Leipzig, 1856. Other important papers on this subject are those of Bessels (1868), and Petrunkewitsch (1901, 1903).

That the eggs laid in such cases in worker cells are in reality fertilized, and those laid in drone cells unfertilized, has been established by direct observation. Von Siebold examined under the microscope 40 fresh laid eggs taken from worker cells; in 30 of these he was able to identify one or more spermatozoa; in three cases the spermatozoa were still moving. In 24 eggs taken from drone cells and carefully examined, no spermatozoa were seen. Von Siebold's observations are fully confirmed by results obtained by the more perfect methods of microscopical study now available. Petrunkewitsch (1901), who has recently made a careful study of the bee's egg by means of sections, found 61 ripe eggs taken from worker cells to have been fertilized, while in only one such egg he found no evidence of fertilization; on the other hand, 273 eggs taken from drone cells were all, with one exception, *unfertilized*. In this one egg the presence of a spermatozoon was indicated. That egg, however, can hardly rank as an undoubted exception to the perfectly obvious general rule. It may well have come from one of the cells of intermediate size on the border between the drone comb and the worker comb, from which either workers or drones may develop, or it may even have been introduced accidentally from worker comb into the lot of eggs in which it was found.

In favor of the view resurrected by Wheeler, that female bees may develop from unfertilized eggs, not a bit of trustworthy evidence has ever been presented, so far as I know. Certainly Wheeler presents none. The strongest support ever given to such an idea came from the experiments of Landois (1867). He transferred eggs from drone to worker cells and *vice versa*, and concluded that the sex of the bee produced depends upon the nature of the cell in which it develops, or more directly upon the character and amount of the food supplied to it. But Bessels (1868), who repeated the experiments of Landois, found that the workers regularly destroy eggs transferred from one cell to another, and the queen then lays new eggs in their stead. Berlepsch, however, as quoted by Bessels, by first removing the queen from the hive, and then by transferring a segment of the cell with the egg attached, succeeded in getting the workers to rear in worker cells a few eggs laid in drone cells. From these developed only drones!

But in the case of ants, Wheeler thinks it even a 'probability' that female ants develop from unfertilized eggs, and the title of his article would lead one to suppose this already a demonstrated fact. His conclusion certainly receives no support from the case of the honey-bee. We may accordingly reasonably ask for pretty clear evidence in its favor from some other source before accepting it as even *probable*. What evidence has Wheeler to present? Three different cases recorded by competent observers, in which worker ants have produced female offspring. But does it follow that the eggs from which these offspring developed were *unfertilized*? Clearly not. Because worker bees do not mate with drones, it does not follow that worker ants never mate with male ants. Indeed, Wheeler quotes Reichenbach, whom he characterizes as 'a very conscientious worker,' to the effect that in at least one species of ant, *Anergates atratulus*, 'fertilization always occurs normally within the nest.' If fertilization may occur within the nest, why may not the wingless worker mate with a male, as does the winged queen? At any rate, this possibility must first be excluded before we shall be justified in drawing the conclusion that all eggs laid by workers are necessarily *unfertilized* eggs. It is a possibility which two of the authorities cited by Wheeler, viz. Tanner and Reichenbach, distinctly recognize. They both emphasize the fact that the workers which under their observation produced female offspring 'had been living in community with males.'

Nor does the third authority, as cited by Wheeler, Mrs. A. B. Comstock, even suggest that the eggs laid by worker ants in the colony kept by her were *unfertilized* eggs. The credit for that idea belongs solely to Wheeler. What she records is that worker ants taken from out of doors laid in captivity eggs which developed into females. Is there any reason for supposing that the ants captured had not previously been with males? If not, then where-

in does this case differ from the other two?

May we not reasonably exercise some 'critical caution' before with Wheeler we conclude it probable 'that worker ants can really produce other workers or even queens parthenogenetically'? But suppose they can; wherein lies the 'ominous import' which such a possibility has for 'current views on sex determination'? For myself, I do not see that the case of the ant would then present any new problems not found either in the case of *Nematus,* or in the silk-moth, or in *Daphnia,* to any theory of sex determination ever conceived or conceivable. Should it be shown that the unfertilized eggs of the ant may develop either into males or into females (at present we have no evidence whatever that such is the case), then it would be in order to inquire whether all such eggs undergo two maturation divisions, as do the eggs of the bee, or whether, as in the Rotifera and Crustacea, male parthenogenetic eggs undergo *two* maturation divisions, whereas female parthenogenetic eggs undergo only one.

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January 25, 1904.

SPECIAL ARTICLES.

AMITOSIS IN THE EGG FOLLICLE CELLS OF INSECTS.

Professor Conklin's interesting account of the amitotically dividing egg follicle cells of the common crickets (*Gryllus pennsylvanicus, abbreviatus* and *domesticus*) in the *American Naturalist* for October, 1903, recalls attention to a condition and phenomenon in animal cytology all too little known. Despite the rarity of amitotic cell division elsewhere among animals, and the interest and significance of the phenomenon, the opportunity offered for its study in the egg follicle cells of insects has been taken little advantage of. We know simply that amitotic division occurs in some of these cells in certain insects. How consistently through the insect class; whether identical or varying in character among the different insect species in which it occurs; and finally and most importantly, how far back in the lineage (ancestry) of the cells themselves the phenomenon persists; in other words, at what time the amitotic division appears in the history of cells which must be derived from cells with mitosis; all these interesting questions remain to be answered. Professor Conklin finds that only in the follicle of the lowest egg-chamber of each ovarial tube of *Gryllus* are all the cells amitotically dividing. In the upper chambers or sections of the tubes the division of the cells is always (as far as observed) mitotic; in the lowest chamber, on the contrary, always amitotic.

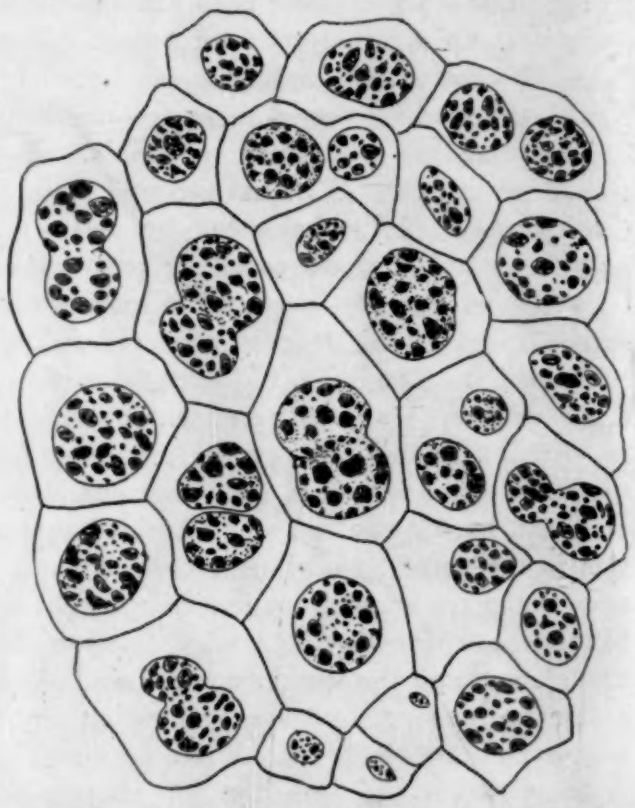

Fig. 1. Egg follicle cells of *Hydrophilus* sp.; showing amitotic division.

The obvious conclusion, in the light of our knowledge of the fate of the follicular cells of the lowest chamber—they secrete here the chorion of the egg and then give up the ghost—that the amitosis is a concomitant with senescence and decay, is probably indisputable. But it is a fact that in not all of the few insects in which this amitosis has been studied is it limited to the follicle cells of the last egg chamber.

Certain differences exist in the character of the amitosis of the egg follicle cells of *Gryllus* (as described by Professor Conklin)

and this process in the follicle cells of *Hydrophilus* sp., the common large water-scavenger beetle of our ponds and stream pools, as I have observed it. In the first place, amitosis is not confined to the lowest egg-chamber; unfortunately I can not say from my present preparations how far up the tube, *i. e.*, toward its germinal chamber, amitosis may be found among the follicle cells, but it is found above the last (lowest) chamber. In the second place, there is no well-defined single nucleolus. So the phenomenon of such a nucleolus surounded by a clear zone and regularly dividing before the nucleus (described for *Gryllus*) is wholly wanting in the follicle cells of *Hydrophilus*. Each nucleus of these cells contains a large number of spherical, strongly staining (chromatin) masses or grains gradating in size from a point up to conspicuously large nucleolus-like balls. The larger of these structures might be looked on as nucleolar masses, if one wanted to use the name nucleolus at all; but if so, from half a dozen to a score of nucleoli would have to be accepted and their only distinction (from the many smaller masses) would be the arbitrary one of size. Fig. 1 shows this disposition and relative massing of the staining (chromatin) substance in the nuclei. No sign of chromatin thread (linin or skein) is apparent. There is absolutely no sign of a division of these nucleoli or chromatin balls accompanying the nuclear division. The nuclei simply seem to be senescent structures with their chromatin content segregated into many small globular masses which vary in size, with all intermediate gradations from small to large. About each of the larger masses a narrow clear zone is apparent.

Fig. 1 shows also the various stages in the simple division of the nucleus and the great size of the nucleus compared with the cytoplasm body containing it. In some cases a cell wall appears between the daughter nuclei after division, but in others the cytoplasm does not seem to effect a clean division, both nuclei then lying, it may be said, in one cell. The actual size of the nuclei averages .04 mm.

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VARIATIONS IN THE PROTECTIVE VALUE OF THE ODORIFEROUS SECRETIONS OF SOME HETEROPTERA.

WHEN on a collecting trip near College Station, Texas, early in October, I examined the stomach contents of a half-grown toad. In this mass I found two stink-bugs (*Euschistus fissilis*). This seemed very interesting on account of the great amount of apparently more palatable insects available here; and especially interesting in connection with the fact that the examinations of the contents of 152 toads' stomachs since the summer of 1901, made to determine what rôle the common squash bug played in the toad's diet during a season when these bugs were exceptionally abundant, revealed less than three per cent. hemipterous material. Mr. Kirkland, in Bull. 46, Mass. Agric. Coll., p. 16, estimated the percentage of hemipterous and dipterous food, after careful examinations of 149 toads stomachs, below two per cent of the whole.

There are variations in the protective efficiency of the secretions of some heteroptera, and this may be an important factor in governing the percentage of diet such insects form with some animals. Many experiments with the common black squash bug were made during the summer of 1901 while at the New Hampshire Experiment Station, and the results there obtained showed that with constant use for a short time the secretions become weaker, and after fifteen minutes are decidedly less effective. Although toads in confinement were witnessed to eat squash bugs, it was only after the secretions were partially exhausted or where the bug was snatched before discharging the liquid. The greatest number a hungry toad would eat was three. In no case was this done in the face of a discharge from the secretion glands.

After twenty-four hours' rest under natural conditions the glands apparently regained maximum strength. This does not apply to hibernating specimens of heteroptera.

On September 15, 1901, half-grown toads were repeatedly killed from the full effects of the fresh discharges from several specimens of squash bugs.

On September 20, 1901, similar experiments required much more time to bring about similar results. Since then I have repeatedly killed half-grown toads in the same manner, while in a number of instances I was unable to kill any.

On October 5, 1903, while carrying 22 cotton boll weevils over a cotton plantation in a vial of 30 c.c. capacity three specimens of *Euschistus fissilis* were suddenly introduced. The secretions were powerfully ejected by these bugs when in the bottle, and in ten minutes the weevils were dead. This experiment was repeated the same day with equal success.

On October 14 and 18, 1903, repeated trials of the preceding experiments resulted in complete failures.

On December 2 four specimens of *Brochymena annulata* were put in a bottle of 45 c.c. capacity, and these killed a blow fly and three stable flies in nine minutes, and quieted a centipede in fifteen minutes, but the latter recovered.

On December 6, 1903, a repetition of the experiments of December 2 showed that twenty minutes more time was required to obtain similar results.

On December 9, 1903, the experiments of December 6 were failures.

On December 13, 1903, four fine specimens of *Brochymena annulata* were found hibernating under kindling wood. With these many experiments were made. They were put in the same vial used December 6 and 9, and when introduced in a warm room the secretions were discharged with much greater effect than in any of the above experiments. Two blow flies and two stable flies, each introduced separately, were killed in five minutes. After twenty minutes a centipede was introduced. Although motionless after eight minutes, the specimen recovered. After this time no more specimens could be killed, although many were introduced.

In all the experiments above referred to the vial was kept tightly corked except when specimens were introduced. Although other insects could be killed, the bugs themselves suffered, apparently, no inconvenience.

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December 15, 1903.

NOTES ON INORGANIC CHEMISTRY.

MENDELÉEF'S CONCEPTION OF THE ETHER.

THE *Chemisches Centralblatt* contains the abstract of a paper by Mendeléef published in the journal *Prometheus* on the subject of an attempted chemical explanation of the ether. From a realistic standpoint it is not satisfactory to ascribe to the ether the properties of weight and chemical individuality. It can not consist of matter now known, disseminated in an exceedingly attenuated condition, because it penetrates all matter, nor can it be the 'Urstoff,' since this would involve the possibility of the annihilation and evolution of atoms. It must rather be considered as a definite chemical substance so light that its molecular velocity is great enough to overcome gravitation; it is without chemical affinity; its power of diffusion is so great that it can penetrate all bodies, and hence can not be weighed, although it actually possesses an extremely small weight. Mendeléef would thus consider the ether to be the first member of the argon group in the periodic system, or what he calls the zero group, and places immediately before the alkali group. By extrapolation he posits an element in this group immediately before hydrogen, with an atomic weight of about 0.4. This he considers possibly identical with coronium. The ether must have a still smaller atomic weight, whose value, owing to this double extrapolation, is extremely doubtful, but certainly can not be over 0.17. For this ether as an element he proposes the name *Newtonium*. That the ether molecule can escape the attraction of the largest bodies of the universe its velocity must be, according to the kinetic theory of gases, at least 2,240 kilometers per second, and from this its atomic weight would be about one millionth that of hydrogen.

By means of this conception, it becomes possible to account for radio-activity, without

having recourse to what Mendeléef denominates the metachemical and vague ('verschwommene') theory of electrons. The radioactive atoms, with their high atomic weights, possess, as large centers of mass, the power of holding a relatively large number of ether atoms, though there is no chemical combination. The entrance and exit of these ether molecules from the groups is accompanied by those disturbances of the ethereal medium which cause the rays of light. The phosphorescence of bodies immersed in liquid air is caused by the increased absorption and condensation of ether molecules at low temperatures. The original article contains many other suggestive thoughts, such as the probability of a fifth halogen element, with atomic weight of about three, corresponding to the fifth metal of the alkalies.

ATMOSPHERIC CORROSION OF ZINC.

A STUDY of the action of the atmosphere upon zinc has recently appeared in the *Proceedings* of the Chemical Society (London), by G. T. Moody. Strips of thin sheet zinc were exposed for five months to the action of the atmosphere, with the result that the metal became completely covered with a half-crystalline coating of a basic carbonate, of formula $ZnCO_3, 3Zn(OH)_2$. From this it appears that the corrosion is to be ascribed, not to a direct oxidation, but to the action of the atmospheric carbonic acid. A confirmation of this was found in the fact that zinc dissolved in a saturated solution of carbon dioxid, the acid carbonate of zinc being formed, and on spontaneous evaporation a precipitate of basic carbonate was formed, of the same composition as that occasioned by atmospheric corrosion. While commercial hydrogen peroxid has a very rapid action on zinc, converting it into the hydroxid, pure hydrogen peroxid, even of thirty per cent. strength, is entirely without action, and the same is true regarding iron. Lead, on the other hand, is rapidly acted on by this reagent, being converted superficially into lead peroxid. Thus the action of the atmosphere on lead may be due to the presence of hydrogen peroxid, but this can not be the case with the corrosion of zinc and iron. That zinc is less corroded in the atmosphere than iron is attributed by Moody to the fact that so much of the carbonic acid is retained by the zinc in the form of basic carbonate, while in iron the carbonic acid is set free as soon as it has done its work, and thus keeps on in its attack upon the iron.

J. L. H.

CURRENT NOTES ON METEOROLOGY.

METEOROLOGICAL PHENOMENA OF THE MONT PELÉE ERUPTION, JULY 9, 1902.

IN the *Popular Science Monthly* for January, Professor T. A. Jaggar gives an account of the eruption of Pelée on July 9, 1902, in which several of the meteorological phenomena associated with the eruption are noted. One of the most striking features of the explosion was a great column of steam observed at 8 A.M. on July 11. "A vertical puff from the volcano rose 10,000 feet into the air, showing at first superb gray-brown cauliflower surfaces, and later taking on smooth outlines, with a funnel-shape and a feathery fringe." A similar steam column was observed on July 16, and a fine photograph was taken of it. The height of this latter column was between four and six miles. As clearly seen in the photograph, the upper part of the cloud was turned towards the east, showing the effect of the anti-trades. On July 9 the tops of a number of *cumulus* (? cumulo-nimbus) clouds were seen to be far below the black dome of volcanic dust. The dust in the air gave the moon a dim reddish-yellow appearance. No strong indraft of air towards the volcano was noted.

DEMTSCHINSKY'S LONG-RANGE FORECASTS.

IN the journal *Climat*, the publication of which was begun in 1901, and to which reference has been made in these notes, Demtschinsky, a Russian engineer, has been making public long-range weather forecasts, based chiefly on supposed lunar influences. These forecasts and the method used by Demtschinsky in making them, have lately been subjected to a critical study by Professor Klossovsky, of Odessa, the director of the Meteorological Service of southwestern Russia

('Examen de la méthode de la prédiction du temps de M. N. Demtschinsky,' Odessa, 1903, 8vo, pp. 74). The conclusions reached by Professor Klossovsky are distinctly unfavorable, as was to be expected. The author suggests that if M. Demtschinsky persists in issuing these forecasts, the whole matter should be taken up by the International Meteorological Committee. Dr. Klossovsky, in connection with his study of the Demtschinsky forecasts, summarizes briefly the present state of weather forecasting, and outlines the older method of mean values, the new method of synoptic meteorology, the use of analytical methods and the question of periodicity.

THE 'LINE STORM' FALLACY.

In the annual summary of *Climate and Crops, New England Section,* a tabulation of daily precipitation between September 14 and 28 at Boston, during 32 years (1872–1903) is given, with a view to throwing light on the popular belief in the equinoctial storm. On September 21 measurable amounts of precipitation occurred but six times during the period, and for the week of which September 21 was the middle day, there have been but twelve years in which the total weekly precipitation was over one inch.

THE CLIMATE OF IOWA.

The 'Annual Report of the Iowa Weather and Crop Service' (Des Moines, 1903) contains an appendix on 'Iowa Climate and Crops,' in which there is a good brief account of the climate of the state (pp. 11–23).

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RECENT ZOOPALEONTOLOGY.

REVISED EDITION OF ZITTEL'S PALEONTOLOGY.

The first volume of the revised edition of von Zittel's 'Grundzüge der Paläontologie,'* which has just made its appearance, is a work of 558 pages covering the whole field of fossil invertebrates. It represents an enlargement of about forty pages over the original edition, with twenty new figures, but except for certain portions relating to the corals and echinoderms, there is essentially no change either in subject matter or in classification. The author remarks in the preface that he has duly considered the merits of the new system adopted in the English version, but has chosen to abide by the older established usage. In the case of the brachiopods and trilobites at least, there are many who will regard this as a backward step, where the studies of Beecher and others have resulted in as satisfactory a classification as exists in the animal kingdom, and it is rather a pity that in the choice of new figures, none of the classic illustrations showing stages of development in these groups were selected. Whether the vertebrate classes will be treated with equal conservatism remains to be seen when the second volume appears.

* 'Grundzüge der Paläontologie,' by K. A. von Zittel, Abth. I., 1903, R. Oldenbourg, Münich and Berlin.

TERTIARY ELASMOBRANCHS FROM SOUTHERN ITALY.

Those interested in the distribution of Tertiary elasmobranchs will find this memoir of Dr. Pasquale,* a student of Professor F. Bassani at Naples, particularly useful, not only on account of the new data it contains, but also because of the careful comparisons the author has made with the type specimens of older writers, resulting in many cases in revised determinations. The various tables given at the end of the memoir are of great convenience. Signorina Pasquale has done for the Italian faunas what Leriche has recently accomplished in praiseworthy manner for the Belgian, in his memoir published by the Brussels Museum, and it is to be hoped that other localities will be taken up by paleichthyologists in similar close detail.

JURASSIC FISHES FROM SPANISH LITHOGRAPHIC LIMESTONE.

Since the discovery made by L. Vidal a year or two ago of the occurrence of lithographic stone in northeastern Spain exactly comparable to that found in Bavaria and central France, a number of fossil remains have been

* 'Revisione dei Selaciane Fossili,' by Maria Pasquale, *Atti Accad. Sci. Napoli,* Vol. XII., No. 2, 1903.

described proving this formation to be contemporaneous with that of Cerin in the Bugey, which is referred by Lapparent and Haug to the summit of the Virgulien. Professor Sauvage,* the eminent director of the Boulogne Museum, now gives us an account of the piscine fauna so far as known from the new locality, which comprises in all thirteen species. The more important of these are illustrated in four photographic plates, amongst the number being a supposed chimæroid egg-case, certainly a very rare petrifaction. It is also interesting to note the presence of *Palæobatrachus* and ichthyosaurs in these beds.

FURTHER LIGHT ON THE TREMATASPIDÆ.

IN an interesting memoir of thirty-three pages published by the St. Petersburg Academy, Professor William Patten,† of Dartmouth, discusses the structure and classification of the primitive family of ostracaphores known as the Trematæspidæ. He describes with painstaking minutiæ the cephalic shield of *Trematæspis schmidti,* illustrating the same with two excellent plates. Our knowledge of this form has been increased by Patten's studies in several important particulars, such as regards the sensory canal system, arrangement of ventral plates, and number of incisions which are commonly regarded as branchial openings, but are interpreted by Patten as having served for the attachment of swimming appendages. Professor Patten's views in regard to arthropod affinities of ostracophores have recently been discussed by Dr. O. Jaekel in the *Zeitschrift der deutschen geologischen Gesellschaft,* and by the reviewer in the *American Naturalist.*

C. R. EASTMAN.

SCIENTIFIC NOTES AND NEWS.

THE University of Pennsylvania has conferred its Doctorate of Science on William Healey Dall, of the U. S. Geological Survey and the U. S. National Museum.

DR. CHARLES S. MINOT, of Harvard Medical School, and Dr. Franklin P. Mall, of Johns Hopkins University, have been made members of the commission for Neurological Research, appointed by the International Association of Academies.

EDINBURGH UNIVERSITY will confer the honorary LL.D. on Dr. Alexander Macalister, professor of anatomy at Cambridge, and on Dr. Hannis Taylor, professor of constitutional and international law at Columbian University.

ST. ANDREWS UNIVERSITY will confer the degree of LL.D. on Dr. J. N. Langley, professor of physiology at Cambridge University.

THE French Geographical Society has awarded its great gold medal for 1904 to Sven Hedin, the Swedish explorer.

DR. NICHOLAS SENN, of Chicago, has been elected a member of the Swedish Medical Association.

ARRANGEMENTS are being made to celebrate the seventieth birthday of Professor Hugo Schiff, the Italian chemist.

PRESIDENT ROOSEVELT has received acceptances from five of those appointed as members of the Isthmian Canal Commission, namely, Rear Admiral John G. Walker, United States Navy, retired; Gen. George W. Davis, United States Army, retired; Col. Frank Hecker, of Detroit, director of transportation during the Spanish-American war; William Barclay Parsons, engineer of the New York subway, and William H. Burr, professor of engineering at Columbia University.

THE Royal Commission on London Traffic have nominated Sir John Wolfe Barry, one of the royal commissioners, Sir Benjamin Baker and Mr. W. Barclay Parsons, consulting engineer to the Board of Rapid Transit Railroad Commissioners of New York, to advise the commission on certain important technical questions connected with locomotion and transport in London.

DR. JAMES CRAUFORD DUNLOP has been appointed superintendent of statistics in the office of the registrar-general for Scotland, in place of the late Dr. Blair Cunyngham.

* 'Noticia sobre los Peces de la Caliza litográfica de la Provincia de Lérida,' by H. E. Sauvage. *Mem. R. Acad. de Ciencias y Artes de Barcelona,* Vol. IV., No. 35, 1903.

† 'On the Structure and Classification of the Trematæspidæ,' by William Patten, *Mém. Acad. Imp. Sci. St. Petersb.,* Vol. XIII., No. 5, 1903.

Mr. H. F. Newall has been appointed assistant director of the observatory of Cambridge University.

Professor Blaserna, of Rome, has been elected a foreign member of the French Physical Society.

Professor A. E. Austin, of the Tufts College Medical School, will pass the next seven months working in Ludwig's Biochemic Laboratory in Vienna.

Dr. D. H. Scott, F.R.S., has been elected president of the Royal Microscopical Society for the ensuing year.

M. d'Arsonval has been elected president of the French Physical Society.

M. Henri Cordier has been elected president of the Paris Geographical Society.

Dr. C. W. Hayes, of the U. S. Geological Survey, is giving a course of six lectures to the geological students of the Johns Hopkins University during the month of February. The lectures embrace a discussion of the structure of the Appalachian district and of the origin of some of the more important non-metallic deposits of economic value in the eastern and southern states.

Dr. Philip Jaisohn, late special adviser to the privy council of the king of Korea, lectured before the Geographical Society of Philadelphia on March 2 on 'Korea and its People.'

At the Royal Institution, London, Professor H. L. Callendar has begun a course of three lectures on 'Electrical Methods of Measuring Temperature,' and lectures have been announced for February 26 by Mr. Alexander Siemens, his subject being 'New Developments in Electric Railways'; for March 4, by Professor W. Stirling, on 'Breathing in Living Things'; and for March 11, by Professor F. T. Trouton, on 'The Motion of Viscous Substances.'

The Academy of Sciences at Berlin has held a meeting to celebrate the birthday of Frederick the Great and of the present German emperor. The principal address was made by Dr. Wilhelm Waldeyer, professor of anatomy.

On February 12, exercises commemorative of the centenary of the death of Immanuel Kant were held at the University of Alabama, at which short addresses were made by Dr. Edward F. Buchner, on the life of Kant and his influence on philosophy; by Dr. H. F. Sayre, on Kant in his relations to astronomy and physical science; by Dr. John Y. Graham, on Kant's contributions to the theory of evolution; by Professor T. W. Palmer, on his contributions to the development of mathematics, and by Mrs. J. Y. Graham, on Kant as a factor in the literature of Germany.

We learn from *Nature* that the hundredth anniversary of the death of Priestley was commemorated in Leeds by the congregation of Mill Hill Chapel, where Priestley was minister for some six years, and also by the Priestley Club. The members of the club, to the number of fifty, dined together, and the president, Dr. T. E. Thorpe, C.B., F.R.S., afterwards gave a public address on 'The Life and Work of Joseph Priestley,' in the Philosophical Hall. At Warrington on the same day, Dr. Thorpe unveiled a memorial tablet at the house which Priestley occupied during his stay in that town.

It is proposed to publish a volume commemorating the work of the late J. S. Budgett, of Trinity College, Cambridge, whose death we were recently compelled to record. A large part of the material collected by him in Africa has not been described, and it is intended that this shall be worked out by his friends and issued under the editorship of Professor J. Graham Kerr.

We regret to record the death of Sir Leslie Stephen, one of the great men of letters of the Victorian era, whose work was largely influenced by the scientific thought of the nineteenth century. His 'Science of Ethics,' published in 1882 and based largely on the theory of evolution, is a scientific work of importance.

The deaths are announced of Mr. W. G. MacMillan, secretary of the British Institution of Electrical Engineers and formerly lecturer on electrical engineering in Mason College, Birmingham; of Dr. Vassili Afanasieff, professor of pathological anatomy in the University of St. Petersburg, at the age of fifty-

five years; and of M. A. Laumonier, the French physician and botanist at the age of sixty-four.

THE senate passed the legislative, executive and judicial, and the agricultural appropriation bills on February 25.

MR. HENRY L. DOHERTY, president of the National Electric Light Association, has offered a gold medal for the best paper on underground construction for alternating or direct current plants.

THE foundation of Schnyder von Wartensee offers its prize of about $700 for an essay on the climate of Switzerland during the last thirty-seven years. Essays, which may be in English, should be sent before September 30, 1906, to the library at Zurich.

Nature states that the Municipal Council of Paris has adopted a proposal of M. Bussat for the foundation of a laboratory of applied physiology. M. Bussat has himself sketched out a scheme of the work which should be undertaken in such a laboratory, relating to the alimentary value of foodstuffs, muscular work, intoxication, etc., and he suggests that the director should give publicity to the work of the laboratory by means of courses of lectures addressed to the pupils of the professional and normal schools of Paris.

Nature states that it is proposed to hold a horticultural and gardening exhibition in the month of June next under the auspices of the Royal Botanic Society in the new exhibition grounds of the society, situated in the center of the Botanic Gardens in Regent's Park, London. The proposed scheme embraces horticulture, forestry, botany, educational methods, nature-study, and a special section for colonial produce. In addition to the exhibition, lectures and conferences are in course of arrangement.

ACCORDING to the London *Times* the British Board of Agriculture, through Mr. Brook Hunt, has asked the governors of the Southeastern Agricultural College, Wye, Kent, to consider a scheme for establishing local field stations for experiments and for providing special courses of training in the processes of agriculture. The board has also suggested the appointment of an instructor in poultry-rearing for the counties of Kent and Surrey. A scheme for establishing school gardens throughout the county of Kent is already under consideration. This, it is understood, has the approval of the Board of Agriculture, and no difficulty in obtaining their sanction for the expenditure of the necessary money is anticipated. The technical education funds of the county will bear the cost of the experiment.

PENNSYLVANIA'S commission for the World's Fair has applied for 3,500 square feet of space for its fish exhibit in the Forestry, Fish and Game Building, and has appropriated $10,000 for the display. A letter from Mr. W. E. Meehan, state fish commissioner of Pennsylvania, announces that he is prepared to put in thirty-five aquaria, as many as the United States Fish Commission will have in its exhibit in the Government Fisheries Building. Pennsylvania's exhibit will also 'include stuffed specimens of mammals, birds and reptiles that prey upon fish life, fishes of abnormal size, legal and illegal devices for taking fish, paintings in colors of the principal food and game fish of the state, state literature upon the subject of fish protection and culture, a miniature waterfall and trout stream, and a hatchery in full operation.'

THE United States Department of Agriculture has just issued 'Farmers' Bulletin No. 189,' 'Information concerning the Mexican Cotton Boll Weevil.' It was prepared by W. D. Hunter, special agent in charge of Cotton Boll Weevil Investigations, Division of Entomology. The work of the Division of Entomology for several years has demonstrated that there is not even a remote probability that the boll weevil will ever be absolutely exterminated. Although the very large yields of cotton of former years may perhaps no longer be possible, it is nevertheless entirely feasible to produce cotton at a margin of profit that will compare favorably with that involved in the production of most of the staple crops of the United States by what have become known generally as cultural methods. These methods consist of modifications of the system of cotton raising

made necessary by the weevil. They were originally suggested by a careful study of the life history and habits of the pest, and naturally any improvement that may eventually be made will be the result of the continuation of that study. They have been tested successfully on a large scale by the division of entomology, as well as by many planters, during two very unfavorable seasons. These methods are in brief as follows: First. Plant early. Second. Cultivate the fields thoroughly. Third. Plant the rows as far apart as experience with the land indicates is feasible, and thin out the plants in the rows thoroughly. Fourth. Destroy, by plowing up, windrowing, and burning, all the cotton stalks in the fields as soon as the weevils become so numerous that practically all the squares and bolls are being punctured. Of greatest advantage is the reducing for the next year of the number of the weevils by the destruction of the plants in the fall. The advantage thus gained is followed by bending every effort toward procuring an early crop the following season. Fifth. While fertilizers are not now used to any considerable extent in cotton producing in Texas, there is no doubt that they should be; not that the land is poor, but that crops may be procured earlier so as to avoid a considerable degree of injury by the weevil, which is more destructive to later crops. The bulletin contains a description of the weevil, the territory affected, and the plan of the investigations by the division of entomology, and gives some of the results of the field work and an experiment showing the damage resulting from favorable hibernating quarters. The bulletin concludes with an account of the legal restrictions concerning the shipment of infested cotton seed and a warning to cotton planters against the inflation of prices of the seed of certain varieties, and the attempts of unscrupulous persons to dispose of common seed from various localities as that of early maturing varieties.

UNIVERSITY AND EDUCATIONAL NEWS.

The presidents of seven New York universities and colleges—Syracuse, Rochester, Union, Colgate, St. Lawrence, New York, University and Hamilton have appeared before the senate finance committee at Albany, to urge the adoption of a substitute instead of the proposed bill appropriating $250,000 for the erection of an agricultural hall at Cornell University.

The supreme court of New Jersey has rendered a mandamus directing the state comptroller to issue a warrant on the state treasury for $80,000 in favor of Rutger's College. The money is due for scholarships established by the legislature, and has remained unpaid for a long time on the contention that the legislative act relating to scholarships was unconstitutional.

Mr. Philip H. Walker has given £1,200 to Oxford University, to establish a studentship in pathology.

The new buildings for the Medical School, the Sedgwick Geological Museum, the Botanical Library and the Law School of Cambridge University were opened on March 1.

On February 19, fire completely destroyed the building at the Ohio State University containing the Departments of Chemistry, Pharmacy, Metallurgy and Mining Engineering. The loss is estimated at $100,000.

The course in economic geology at The University of Chicago this year consists of a double study for twelve weeks. The course is divided into two parts: 'The Non-metallic Mineral Resources,' and 'The Metallic Mineral Resources.' The first part was given Dr. E. R. Buckley, director of the Missouri Bureau of Geology and Mines and the second part is being given by Dr. H. Foster Bain, geologist of the U. S. Geological Survey.

The following have been appointed electors, at the University of Cambridge, to the professorships indicated: chemistry, Professor J. J. Thomson; anatomy, Sir M. Foster; botany, Professor Clifford Allbutt; Jacksonian (chemistry), Sir William Ramsay; Downing (medicine), Sir M. Foster; zoology, Mr. J. W. Clark; physics, Professor R. B. Clifton; physiology, Professor Clifford Allbutt; surgery, Sir Frederick Treves, Bart.; pathology, Professor R. Muir.